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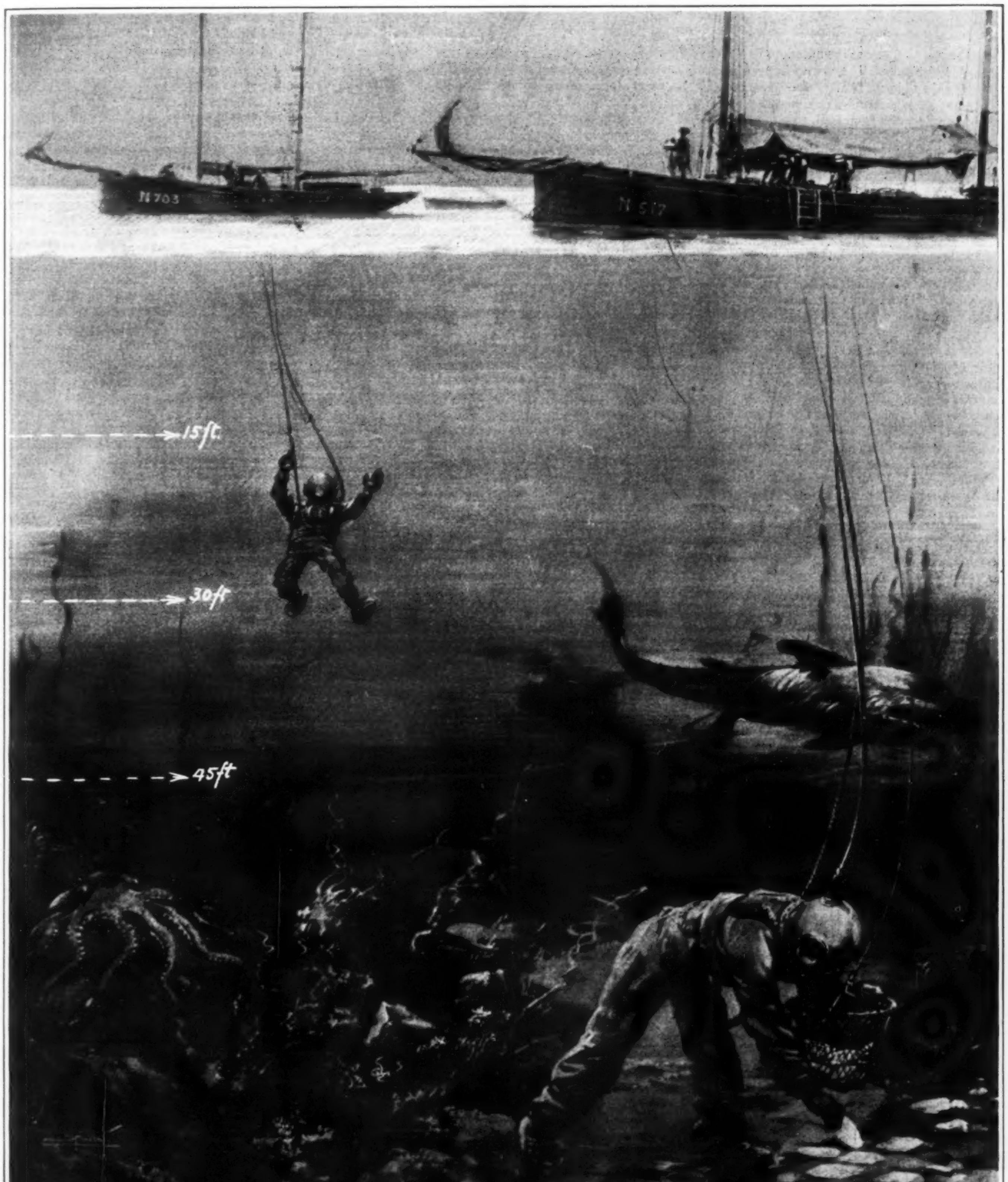
SCIENTIFIC AMERICAN SUPPLEMENT

VOLUME LXXVII
NUMBER 1983

NEW YORK, JANUARY 3, 1914

10 CENTS A COPY
$5.00 A YEAR



By courtesy of The Illustrated London News.

Diver gathering pearl oysters in Western Australia.

BIOLOGICAL SCIENCE AND THE PEARLING INDUSTRY.—[See page 12.]

Heredity*

Considered Especially With Reference to Defective Conditions of Body and Mind

By W. Bateson, M.A., F.R.S.

THE MENDELIAN PRINCIPLE.

The essence of Mendelian principle is very easily expressed. It is, first, that in great measure the properties of organisms are due to the presence of distinct, detachable elements, separately transmitted in heredity; and secondly, that the parent cannot pass on to offspring an element, and consequently the corresponding property, which it does not itself possess. The determination or recognition of these elements by analytical breeding is one of the main objects of present-day genetic research. Each germ cell, ovum, or sperm may contain or be devoid of any of these elements; and since all ordinary animals and plants arise by the union of two germ cells in fertilization, each resulting individual may obviously receive in fertilization similar elements from both parents, or from neither. In these cases the offspring is "pure" bred for the presence of the character in question, or for its absence. But it may be formed by the union of dissimilar germs, one containing the element, the other devoid of it, and in this case we call the individual cross-bred, or heterozygous in that respect.

A population thus consists of three classes of individuals, those pure for the presence, having received two doses of an element; those pure for the absence of the element, having received none of it; and the cross-breds, which have received one dose only. From the physiological standpoint it is of great importance to distinguish the positive from the negative characteristic. But since it often happens that the full effect is produced in the organism by one dose only of a character, we have felt justified in regarding this positive effect as the sign or proof that it is caused by something present. A plant, for example, though crossbred for tallness, may be as tall as one pure bred for tallness. Each dwarf plant, whatever be its parentage, can only produce dwarf offspring. Not having tallness it cannot transmit that property. A crossbred tall plant can, by self-fertilization, produce both tall and dwarf offspring. Fowls with silky feathers cannot, if bred together, have offspring with normal feathers, but two birds, normal to all appearance can, if they be crossbred in that respect, produce silky offspring.

At a later stage we will consider to what extent we are justified in thus discriminating between the positive and the negative characters, but for the present we will note that, as a matter of symbolical expression, the distinction is immaterial. Now, the organism produces germ cells representing in equal number the characteristics of the germ cells whence it arose. There is, as we call it, segregation between the elements introduced from the parent. The double structure of the soma is, when the germ cells are formed, resolved into the single structures of the germ cells. If the organism is pure bred in any respect, all its germ cells are alike in that respect. If it is crossbred, its germ cells represent in equal numbers the positive or negative elements which were brought together in fertilization. In consequence, the numbers of individuals resulting from the various possible forms of union follow regular arithmetical rules, and the proportions in which the several classes appear give us the means of tracing and identifying the various systems of descent.

CONDITIONS SHOWING DOMINANT DESCENT.

It was naturally with great interest that we began the examination of human pedigrees, and the result of that search soon showed that many of the more definite hereditary diseases and malformations follow one or other of the systems with which Mendelian analysis has familiarized us. The first condition thus recognized to have a simple Mendelian inheritance was the peculiar malformation known as brachydactyly, described in America by Farabee. Another example is congenital cataract studied by Nettleship, to whose admirable researches we owe many of our most valuable and instructive pedigrees. Both these conditions descend as dominants. It is characteristic of them that unaffected members of the families do not transmit. Not having in them the critical element which causes the condition, they cannot pass it on to their posterity. In the human examples the individuals affected are almost always heterozygous, and hence among the children which are born to their marriages with normal persons we expect the affected and unaffected to be in equal numbers.

Since then such pedigrees have been recognized in abundance. The collection of such evidence we owe especially to Nettleship, Gossage, Drinkwater, Lundborg, Weil, Bulloch, and to many more also. Some of the conditions, which may be stated with fair confidence to descend commonly as dominants, are the following:

Brachydactyly, claw-shaped extremities, cartilaginous exostosis, membranous cranio-cleido-dysostosis, hypoplasia of the teeth, diabetes insipidus, chronic trophoedema (generally).

Skin affections: Tylosis, epidermolysis bullosa, telangiectasis, hypotrichosis, porokeratosis, and xanthoma.

Opthalmic diseases: Pre-senile cataract, ectopia lentis (sometimes), coloboma (sometimes), distichiasis, night-blindness (certain forms), and retinitis pigmentosa (sometimes).

Nervous diseases: Angio-neurotic œdema, tremor hereditarius, Huntington's chorea, ptosis, and probably several of the forms of spastic paralysis and of myoatrophy.

Respecting nearly all these it may be remarked that exceptional cases, many undoubtedly authentic, are on record in which affected children have been born to parents actually or apparently normal. Many of these exceptions are undoubtedly genuine and serious, but others may be ascribed to irregularities in the age of incidence, and to mistakes as to slight cases.

Another difficulty arises from the fact that the numbers are frequently irregular. This is in part caused by imperfection in the records. In part, however, I think these aberrations point to real peculiarities in the process of segregation. We know well that in certain plants like wheat and peas the expected numbers appear with great constancy, but in others, as stocks and the primulas, long "runs" are so common that smooth results are only obtained by the summation of large series of observations. We are perhaps right in conceiving the germinal tissue of a heterozygote as an emulsion of the two types, sometimes coarse, sometimes fine, and the degree of smoothness of the results may be an indication of its state in this respect.

In regard to a large group of diseases of nervous origin, such as Friedreich's disease, Thomsen's disease, and others, these sources of difficulty are especially serious, and though from study of many genealogies I have come to the conclusion that most of them are essentially dominants, I offer this opinion with reserve.

RECESSIVE CONDITIONS.

Of recessive conditions in man we have less abundant evidence. Inasmuch as they usually appear from the unions of parents both apparently normal, though heterozygous for the condition, their occurrence is rare and sporadic. Lundborg has shown with great probability that paralysis agitans is one of these. The constant intermarriage of families in the valleys of Sweden, Norway, and the Alps, gives the best opportunity for the study of this form of descent. Lately, also, the American students of genetics have produced evidence making it clear that feeble-mindedness has at least one of the marked features of a recessive condition. When both parents are feeble-minded they have no normal children. It is nevertheless difficult to regard this condition as a simple recessive, for unions of the feeble-minded with normal parents almost always produce some feeble-minded children. Among our experimental families of plants and animals there are mysterious examples somewhat comparable with these, but the exact nature of the complication is still obscure.

Other conditions which exhibit a behavior characteristic of recessives are albinism, myoclonus, epilepsy, and alkaptonuria. They may all appear in the children of normal persons, with special frequency as the result of marriages of related parents, and there can be no reasonable doubt that these conditions are due to the loss of some factor present in normal persons.

At this stage attention should be called to a remarkable group of cases in which the appearance of abnormality may almost without doubt be regarded as partially dependent on the conditions to which the soma is subjected. Of these I may mention in particular some of the congenital abnormalities, such as hallux valgus, and some of the congenital deformities of the joints, as, for example, dislocation of the hip or of the radius. In these cases we meet the curious fact that whereas the condition most usually appears in the offspring of normal parents, it may, in certain families, be transmitted from parent to offspring as a dominant. I am inclined to compare these phenomena with those so often witnessed in incubating eggs. When the incubators are not running uniformly many of the chickens are born with deformed feet. Such abnormality, however, is found with special frequency in particular strains of birds, though eggs from other strains exposed to the same conditions may give perfectly normal results. The liability is the thing transmitted, but without the appropriate conditions the effect is not produced.

DESCENT OF SEX-LIMITED TYPE.

The next group of cases to which I would call your attention is one in which the descent of the abnormality is limited wholly or partially by sex. Of these the best known examples are those of the descent of color-blindness, hæmophilia, and one of the forms of nystagmus.

In regard to color-blindness, the fact has long been known that males are affected with exceptional frequency. Color-blindness among women is decidedly rare. Horner was, I think, the first to call attention to the fact that in the pedigrees of this condition there is remarkable regularity. Normal women transmit the affection to their sons, whereas color-blind males with rare exceptions have only normal offspring. It is possible to interpret these phenomena by the suggestion to which several of us inclined some years ago, that color-blindness is a dominant, inasmuch as the normal males cannot transmit it. We are led to regard the condition as due to the presence of something inhibiting normal color sense rather than to the absence of the factor to which that sense is due. Further investigation has, however, thrown doubt on this simple account. As to the descent, however, there is no doubt that the sons of color-blind males do not inherit the peculiarity, and therefore cannot transmit it. The daughters of color-blind fathers inherit it, and though it does not appear in them, probably all of them have the power of transmitting it to their sons. Knowledge derived from direct experimental breeding of various animals has provided a very remarkable scheme by which this course of descent can be represented factorially. One of the simplest of such examples is that of the inheritance of color in Sebright bantams. When a cross is made between a golden hen and a silver cock, all the chickens come silver, but when a reciprocal cross is made, a silver hen being mated with a golden cock, the pullets come golden but the cockerels come silver. In this case we must regard the silver as dependent on the presence of dominant factors, and the golden as due to its absence. The silver male is pure for silver SS, having received that factor from both parents, but no pure silver hen exists. The hen is heterozygous for silver and may be represented as Ss. We must further regard the hen as also heterozygous for femaleness, Ff. In the formation of the eggs there is what we call repulsion between the factor F and the factor S; the two are not present together in any egg. Those eggs which are destined to become males carry factor S, and those eggs which are destined to become females carry factor F. Now, in color-blindness the repulsion that has been so clearly proved between these factors may be represented as acting in the germ cells of the male between the factor for maleness and the factor for color-blindness; such that the sperms destined to become males carry factor M but do not carry the factor for color-blindness, which passes entirely into those sperms which are destined to produce females. It is evident that females heterozygous for color-blindness are not color-blind, and that color-blind females can only be produced by the meeting of two germ cells both bearing the affection.

It follows from the hypothesis that the sons of color-blind women will all be color-blind, and all the records of such families with which I am acquainted, with a single doubtful exception, are in agreement, in that the sons of such women were all color-blind. It is practically certain that the same system of descent holds for a form of nystagmus, respecting which Nettleship has recently collected some important genealogies. As to the other sex-limited conditions in man I can only say a few words. Hæmophilia is the most familiarly known of these. It is likely that the condition in men follows the same descent as that established in the cases of color-blindness, but inasmuch as males affected with the condition very rarely live to the age of puberty, little is known of their powers of transmission.

I have already spoken of night-blindness as a condition which descends as a dominant, but it is very interesting that Nettleship has lately published certain pedigrees of night-blindness in which the descent is of the sex-limited type. This particular night-blindness is apparently associated always with a high degree of myopia, and I suppose there can be little doubt that the recognition of the genetic distinction will lead to the discrimination of pathological differences also.

THE QUESTION OF INHIBITING FACTORS.

At an earlier stage of this lecture I mentioned a doubt as to the justice of the inference that the characteristics which behave as dominants are due to the presence of factors, and to that point I must now return. In our experimental studies of animals and plants we are award that certain negative characteristics may be produced

* Address delivered before the Seventeenth International Congress of Medicine and published in the *Lancet*.

not by the absence of a quality, but by the presence of some factor which inhibits its expression. In illustration I may give as an example the Chinese primrose. These plants may have their flowers colored or white, and we are aware that the white may owe their whiteness in certain cases to the absence of color, in which case, on a cross with the colored form, the offspring are all colored; whereas other whites contain something which destroys or inhibits the development of the pigment factors contained in them.

In view of these examples the question fairly arises whether some of the dominants whose descent I have traced in human pedigrees may not in reality be due to the absence of some factor which in normal persons prevents such development. This suggestion was prominently brought forward by Doncaster in regard to color-blindness. He lately suggested a scheme according to which we should regard all normal males of the population as heterozygous Nn for the factor N normality, and all ordinary females as homozygous NN for the same factor. In either case the course of the descent would, both qualitatively and numerically, be the same. Between these two methods of representation we cannot as yet distinguish. The point is of great importance in another respect. The question will be prominent in most of your minds, whence do these dominant factors come? Whereas it is comparatively easy to conceive some process by which some elements of the composition may be lost, we are quite unable to suggest any source from which a factor may be derived or any mode by which it may be taken into the genetic system of the organism. We have been accustomed to speak of factors as being added to, or subtracted from, the sum total of an individual composition, but as to the actual origin of new dominants there is very little contemporary evidence.

In the case of those animals, such as the fowl, the wild original of which we think we know, many factors exist in our domesticated breeds which are not possessed by any of the wild species. No wild species, for example, has the complicated combs that we know in our domesticated breeds. None has the dominant white color of the leghorn; and many other such instances might be given. How have these elements been added to the composition of the fowl?

The problem is exactly parallel to that constituted by the occurrence of brachydactyly or night-blindness in man. We can only trace back these peculiarities to a single individual of whose origin nothing is known, but whence did he derive the factor to which he owes his peculiarity? We may compare these cases with those of the origin of an infectious disease, such as typhoid fever. When a medical officer of health recognizes typhoid in his district he knows that it must have come from some definite source of infection, and but for one feature in the descent of dominants we might be tempted to suggest that they also arose through the introduction of some element from without into the system. I see no *a priori* impossibility in such a belief, but in these dominants we know that the distribution among the germ cells is approximately symmetrical, and nothing that we know of animals and plants justifies the suggestion that a foreign element can be so treated in gametogenesis. Perverse as such a suggestion may appear, I do not think we should close our minds to the possibility that these dominants arise by a process of loss of some inhibiting factor. Until we have some far more direct method of recognizing the presence of factors this suggestion cannot be positively gainsaid.

Let me call your attention also to the inference which this suggestion would have on our conceptions of evolution. We might extend the same reasoning to all cases of genetic variation, and thus conceive of all alike as due to loss of elements present in the original complex. For all practical purposes of symbolic expression we can still continue to use in our analyses the modes of representation hitherto adopted, but we must not, merely on the ground of apparent perversity, refuse to admit that the lines of argument here indicated may prove sound.

INHERITANCE OF NORMAL CHARACTERISTICS.

Much as we now know of the inheritance of these abnormal characters, some common, others rare, we know little of the descent of the normal characteristics of an ordinary human population. Indeed, the only characteristics as to the genetic nature of which we have clear evidence are the color of the iris and, to some extent, the color of the hair. Our knowledge of these is derived from the work of Hurst; he it was who first succeeded in determining the inheritance of eye color, which had long been a puzzle to anthropologists. The facts which he collected clearly showed that the presence of pigment on the front of the iris, the condition commonly known as brown or black eyes, is a dominant, whereas the absence of this pigment, the eyes we variously call gray or blue, is a recessive. And so far, though I anticipate the possibility of exceptions, no example has yet been produced of two parents critically known to have blue eyes producing offspring with brown or black eyes. As similarly shown, red hair behaves as a recessive.

From the records of portraits of the Royal house of Hapsburg it is clear that the familiar facial peculiarities of the family behave with fair regularity as a dominant, and there are indications that several of the artistic gifts follow with some accuracy the course characteristic of recessives. By careful observation and record it would undoubtedly be easy to extend the list.

DESCENT OF INSANITY AND OTHER CONDITIONS.

Referring to the descent of various forms of insanity nothing has been made out with certainty with the exceptions that I have mentioned: hereditary chorea, which behaves as a dominant, and the condition somewhat vaguely described as feeble-mindedness, which behaves as a recessive. The genetic analysis of insanity is at the present time practically impossible from the fact that the diagnosis of the various forms of insanity is by no means clear, and also from the fact that the conditions of life have obviously much to do with the development of such weaknesses. Similar difficulties arise in regard to deaf-mutism. It is not in question that this condition is very often transmitted, but forms of deafness are so various, and there is such uncertainty as to the nature of the condition in any given case, that we cannot expect to perceive the operation of any regular system here. It is not impossible, also, that in regard to the descent of liability to infectious disease we may hereafter trace in man rules similar to those which have now been established in certain plants, especially by Biffen.

THE NATURE AND TIME OF SEGREGATION.

I propose now briefly to allude to some other significant aspects of these results. First, as to the nature of segregation. This, I think, we must regard as a process comparable with the mechanical separation of substances which will not mix, or mix imperfectly; whereas some factors are continually transmitted in their entirety, others are liable to be broken up by what I regard as a process of quantitative fraction occurring in the mechanical dissociation of the elements at certain critical cell divisions. As to which are the critical cell divisions we have no clear indication. I cannot agree with those of my colleagues who think segregation must occur exclusively in the maturation processes. The case of the double Stock, in which the whole male side of the plant differs genetically from the female side, as proved by Miss Saunders, shows almost conclusively that segregation may occur in somatic divisions. It is also difficult otherwise to interpret the fact that in certain cases the parental combination influences the distribution of factors among the gametes so that the distribution among the grandchildren is different according to the way in which the characters were combined in their grandparents. Into these details I cannot now enter.

THE DEFINITENESS AND FIXITY OF THE LAWS OF DESCENT.

The tendency of our work, as you will perceive, is more and more to exhibit the definiteness and fixity of the laws of descent. The medical man, justly proud of what he can do to make good bodily shortcomings, looks naturally to the conditions of life as the influences which chiefly determine destiny. The statesman, to whom physiological fact is a mystery that he rarely feels any desire to explore, inclines naturally to a similar conception of life. There are signs that more true views are beginning to prevail. It is impossible to study such pedigrees as those of night-blindness, in which a condition introduced by one individual into a community hundreds of years ago continues to be perpetuated according to definite arithmetical rules among a remote posterity, without being impressed with the fact that whatever influences may be brought to bear by hygiene or by education the ultimate decision rests with the germ cells. Evolutionary change is effected not so much by gradual transformation of masses under ameliorating or detrimental conditions, but in the main by the occurrence of individual and sporadic variation.

EUGENICS AND LEGISLATION.

It is beyond my province to discuss in detail the practical question denoted by the word eugenics. Those who are engaged in the work of physiological analysis probably do well to keep clear of those distractions. The direction, however, in which genetic research points is not difficult to determine. Sir James Crichton-Browne has discussed at this Congress the striking percentage increase of lunacy in recent decades, and, as always when such topics are under consideration, there are persons disposed to lay the blame on the conditions of life, the severity of the modern struggle, a greater or less consumption of alcohol or other drugs, and so forth; those, however, who have some knowledge of genetic physiology are aware that the whole force of modern science and legislation has hitherto been exercised in the preservation of defective strains in our midst, and will not feel serious hesitation as to the true cause of the increase.

The Mental Deficiency Bill we recognize as, in principle, a wise beginning of reform, but, on the other hand, we cannot hear without disquietude of the violent measures that are being adopted in certain parts of the United States with similar objects. It is one thing to check reproduction of hopeless defectives, but another to organize a wholesale tampering with the structure of the population, such as will follow if any marriage not regarded by officials as eugenic is liable to prohibition. This measure, we are told, is actually proposed in some of the States in America. Nothing yet ascertained by genetic science justifies such a course, and we may well wonder how genius and the arts will fare in a community constructed according to the ideals of State Legislature.

Philologists tell us that by an irony of development the word "dull" comes from the same original which in Dutch has become "dol"—mad, and is better known to most of us in the German equivalent "toll." But I anticipate that, connected as the ideas may be, we might by ridding our community of mania leave it gravely infected with dullness. There are other no less obvious considerations which I might develop, but I trust I have said enough to show that in the experimental analysis of genetics we have an instrument of novel and extraordinary power, and I am not alone in foreseeing that genetic science must profoundly influence the course of human thought and ultimately the conduct of society.

Darwinism One Hundred Years Ago

H. Gadow, writing to *Nature*, makes an interesting contribution to the history of the theory of evolution. Says the author:

"Who was the first to propound clearly the idea of sexual selection as an important factor in evolution? 'Darwin, of course,' is the usual answer, even of those who, sneering at this great man, delight in pointing out that it was not he who first promulgated the improving effects of selection, and that all he himself did introduce was the subsection of sexual selection; according to them a baseless idea.

"Recently I happened to come across the following statement by Friedrich Tiedemann, in his 'Anatomie und Naturgeschichte der Vögel' (Zweiter Band, p. 13, Heidelberg, 1814): 'Very often there arise fights between the males for the possession of the females. . . . These fights, which take place also between very many mammals, seem to be very important for the conservation of a healthy progeny, since only the strongest and most vigorous males propagate the race, while the young and too old individuals, being weak, are conquered, and removed from propagation.

"Tiedemann, who flourished just one hundred years ago, was a zoologist with great and clearly expressed ideas, and the following quotations may be of interest:

"'Metamorphosis of the Birds.' There is a metamorphosis concerning the whole life of the individual bird, from the moment of hatching to its death. There is further a yearly metamorphosis, culminating with the period of propagation; and a less significant diurnal change. Lastly, *there is a metamorphosis due to successive geological epochs*" (pp. 288-325).

". . . With every larger geological epoch (Erd-Revolution) some animals have perished. . . . But it seems also that after each of such revolutions new animals have been formed, mainly, I suppose, through gradual metamorphosis and alteration of the previous remaining animals into new kinds (Thierformen), caused by new climatic and physical influences" (p. 322.)

". . . These fossil rests of birds testify to the age of the class of birds. But since all these remnants seem to belong to extinct kinds of birds, they can be taken as proofs that in the course of time the species is just as much subject to metamorphosis as the individual (p. 325)."

Mr. A. Dendy draws attention to a passage in Erasmus Darwin's "Zoonomia" (first edition, 1794; quotation from 1800 edition):

"A great want of one part of the animal world has consisted in the desire of the exclusive possession of the females; and these have acquired weapons to combat each other for this purpose. . . . So the horns of the stag are sharp to offend his adversary, but are branched for the purpose of parrying or receiving the thrusts of horns similar to his own, and have therefore been formed for the purpose of combating other stags for the exclusive possession of the females; who are observed, like the ladies in the times of chivalry, to attend the car of the victor. . . . The final cause of this contest among the males seems to be that the strongest and most active animal should propagate the species, which should thence become improved."

Construction of a Fifty-Watt Transformer*

Directions for the Amateur Electrician

By Charles F. Fraasa, Jr.

The comparative ease of transforming the current of the commercial alternating current lighting circuits to lower potentials by means of a small transformer makes it especially adaptable and practical for such uses as: ringing door bells, lighting low voltage sign lamps, and

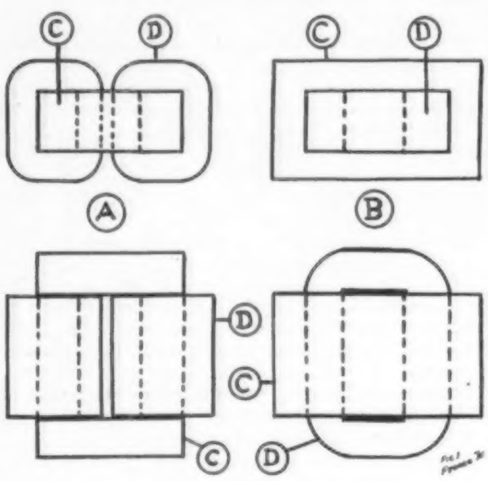


Fig. 1.—Types of transformers.

for experimental purposes. The small transformer is a very practical substitute for dry batteries; and its efficiency is higher than can be attained by any form of resistances on lighting circuits to reduce the voltage.

The transformer consists essentially of two coils, each forming a separate electrical circuit, wound upon an iron core. The current to be transformed is impressed upon one coil, which is known as the primary. The

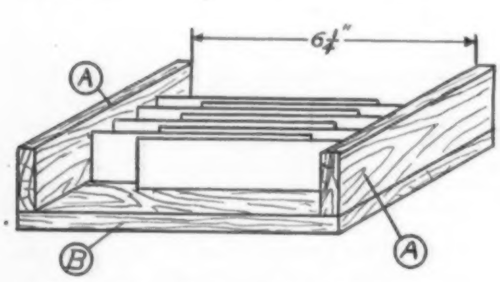


Fig. 4.—Form for assembling the core legs.

current in the primary produces a magnetic flux in the core, which in turn induces a current in the other coil, known as the secondary.

The ratio of the primary impressed electromotive force to the secondary induced electromotive force is directly proportional to the ratio of their number of turns. A transformer receiving a current at a low potential, and delivering a high secondary potential is known as a step-up transformer; a transformer receiving a current of high potential and delivering a current of low secondary potential is known as a stepdown transformer.

With reference to the arrangement of the core and coils there are two general types; namely, the core type, and the shell type. The arrangement of the core and coils in the two types is shown in Fig. 1, in which *A* is the core type, and *B*, the shell type. In the core type transformer *A*, the core *C* is of a rectangular shape, and the coils *D* are wound upon the side members of the rectangle. The shell type is just the reverse; the coil *D* being rectangular in shape, and the core *C* forming two circuits, one around each limb of the coil. The illustration shows that in the core type transformer, the coils surround the core, while in the shell type, the core surrounds the coils.

The transformer design given in this article is for a 50-watt step-down transformer of the core type, stepping down 110 volts alternating current at 60 cycles to various lower voltages. Data for two windings will be given, one for five and ten volts, 5 amperes; and one for 25 volts 2 amperes. By a suitable arrangement of taps the 25 volt 2 ampere winding can be arranged to give a secondary voltage of from 2.5 to 25 volts in steps of 2.5 volts, the following voltages being available; 2.5-5-7.5-10-12.5-15-17.5-20-22.5-25. The 5-10 volt winding will be suitable for ringing large bells, lighting small sign lamps, and operating electrical novelties and toys, while the 25 volt winding will furnish a wide range of voltage for experimental purposes.

The core is composed of a number of laminations of thin sheet steel *A* and *B* laid in the shape of a rectangle, as illustrated in Fig. 2. The first layer is laid as shown on the left hand side, and the second as on the right hand side. In assembling the core, these layers are alternated. By this means the core can be divided con-

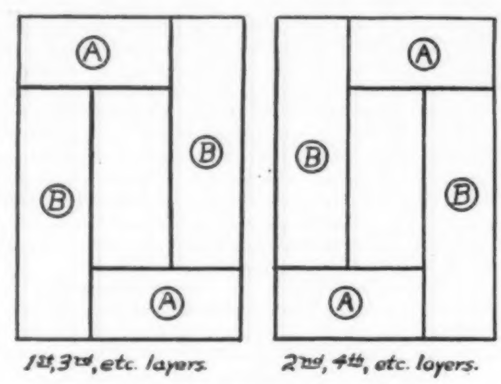


Fig. 2.—Showing alternate layers of core.

veniently for winding the coils, and the air-gaps at the joints effectively reduced to a minimum. With the core assembled in this way, it will be found that the core legs *B* are composed of alternate strips projecting on the ends for a distance equal to the width of the strips composing the core. The legs may then be assembled separately, and when the coils are wound in place, the yoke strips *A* may be built across, filling in the interstices of the leg ends.

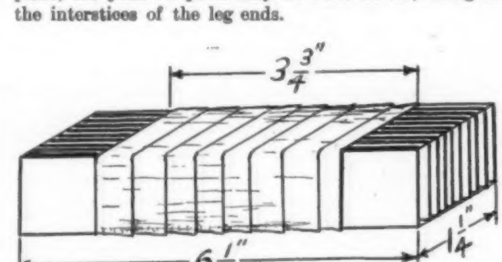


Fig. 5.—Core leg ready for winding.

For the core, ordinary stove pipe iron will do very well. The design was made with a view of using this grade of iron. But if one of the better grades of transformer iron is available, it should be used, as this will cut down the core loss, and so increase the efficiency of the transformer.

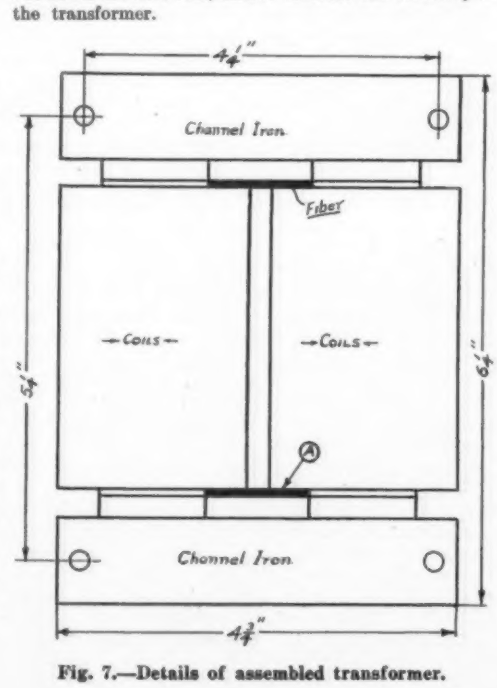


Fig. 7.—Details of assembled transformer.

The core laminations are dimensioned at *A* and *B* Fig. 3. Enough of each dimension should be cut to make a stack about 2½ inches high when tightly compressed. Before assemblying the core, it is well to insulate the laminae from one another. This may be done by coating one side of each strip with thin shellac or japan. Another method is to dip half the strips in insulation and alternate them with the remaining strips when assembling the core.

In the construction of the core, the legs will be assembled separately; when the coils are wound on the legs the yoke strips will be built across the ends. The legs are to be assembled in a form Fig. 4, which will

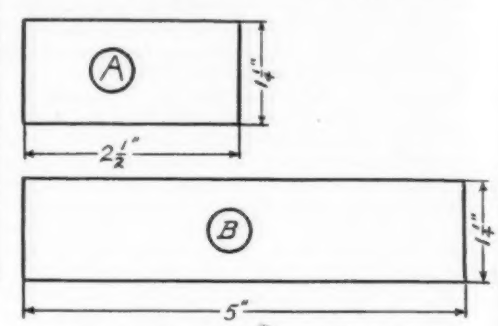


Fig. 3.—Dimensions of core laminations.

greatly facilitate the assembly. This form is made by fastening two strips of wood *A* on a piece of board *B*, or on the bench, spacing them 6¼ inches apart. Be sure that the strips are fastened down parallel. The core legs are then assembled by placing alternate strips first against the block on one end, and then against the other end. This will leave 1¼ inch square interstices for the yoke strips between the alternate leg strip ends. When enough strips have been assembled to make a core 1¼ inches thick, grip them tightly between

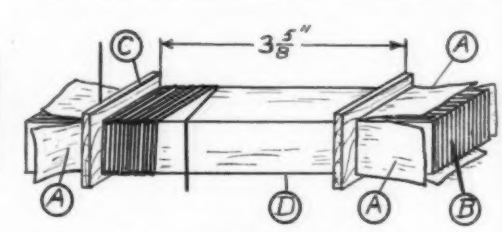


Fig. 6.—Method of winding coils.

the thumb and the fore finger, and fasten them securely in a vice. The core should then receive a layer of friction tape, wound on very tightly, when it will appear as in Fig. 5.

The core is prepared as in Fig. 6 for the winding of the primary and secondary coils. A strip of heavy duck *A* is laid on each side of the core *B*, and a wooden collar *C*, cut from some ½ inch wood is slipped on over each end. These pieces should be spaced 3 5-8 inches apart. Then wind several layers of heavy wrapping paper *D*, in the space between the two collars *C*, and apply a liberal coat of shellac to each layer.

The primary coils are then wound on the core, being careful to wind evenly and in layers. Between layers wind a layer of empire cloth or tough paper. Over the completed primary winding wrap about six layers of empire cloth, and wind the secondary. This should also be wound evenly and in layers, being careful to bring the taps out at the proper turn. It would be well to wind a layer of empire cloth between the layers of wire, and to apply shellac liberally as the winding proceeds. When the coils are wound, remove the blocks *C*, Fig. 6, from the coil ends, and bend back the ends *A* of the strips of duck which were placed on the core before winding. These ends should be pulled back very tightly over the winding and bound in place by a layer of friction tape. A layer or two of heavy duck over the whole coil, shellaced liberally completes the coil.

The leads to the beginning and end, and the taps on each coil should have a short piece of flexible cord or lamp cord soldered to them for connections.

The windings are as follows:

Primary: 430 turns No. 23 d.c.c. per core, total 860 turns.

Secondary: (1)—10 volt 5 ampere, 44 turns No. 13 d.c.c. per core, total 88 turns.

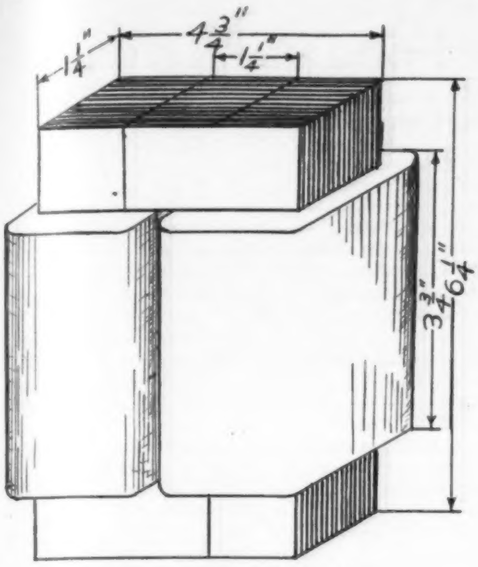


Fig. 8.—Assembled transformer.

(2)—25 volt 2 ampere, 110 turns No. 17 d.c.c. per core, total 220 turns. The 2.5 volt taps consist of 22 turns, and the 5 volt taps of 44 turns. Tap 1, leg 1, turn No. 22; tap 2 leg 1, turn No. 66; tap 4 leg 2, turn No. 22; tap 3 leg 2, turn No. 66. See Fig. 10 for illustration.

When both cores are assembled, and the coils are wound, set the cores up on end, spacing them so that the core ends will be 1¼ inches apart. On top of the coils lay a piece of fibre or fuller board *A* Fig. 7 to protect the winding when the yoke strips are put in place. Then put the first strip in the space between the first two leg strips, flush with the outside end of the leg. The strip will then project over to the other core leg. The second strip is put in from the other side in the same manner. Build the end across this way until all the interstices between the core ends are filled. The core will then

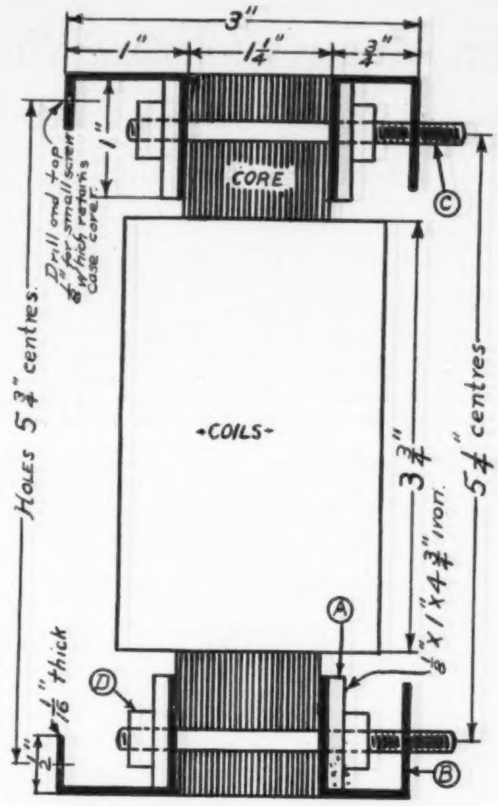


Fig. 9.—Details of transformer mounting.

be a solid mass of iron. Turn the core over and fill in the other end in the same way. The assembled transformer will then appear as in Fig. 8.

With a piece of fibre and a light hammer, drive the yoke strips all flush with the surfaces of the core ends. The yoke ends should be clamped together to prevent vibration and humming while in operation. This is done by means of the iron strips *A* which are dimensioned in Fig. 9. The channel irons *B* made of 1-16 inch sheet iron are for the purpose of mounting the transformer in its case. The angle irons and the clamping strips are fastened together by means of the bolts *C* and the nuts *D*. Figures 7 and 9 show the transformer ready for mounting in its containing case.

Figure 10 shows how the coils should be connected The connections between the two coils should be such that if a direct current were circulating through them, it would flow in opposite directions around the two cores. This applies to both the primary and the secondary coils. The proper connection may be effected by connecting the ends of the coil on one core to the end *E* of the coil on the other core, provided that they are both wound in the same direction. The illustration, Fig. 10, shows what voltages are obtainable between the various taps.

Fig. 11 dimensions the containing case which is made of No. 22 gage sheet iron. The holes *A*, punched in with a blunt ended tool, are for the purpose of giving access to air for cooling the transformer. The sides of the case consist of a plain band of iron bent to rectangular shape, and riveted at one end. The top and bottom are bent down on the four sides as *A* Fig. 12. The

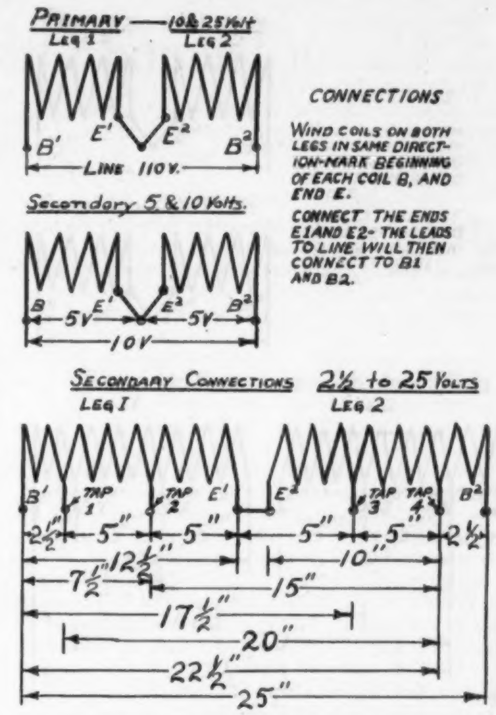


Fig. 10.—Connection diagram for 50-watt transformer.

bottom may be riveted along this edge, but the top is fastened in place by the screws, *B* Fig. 12, through the channel irons *A*, Figs. 7 and 9.

At one end of the case, a rectangular hole 3 by 1½ inches is cut through for the leads to the secondary terminal plate, Fig. 13, which is mounted over this hole. The primary terminal plate is mounted over a similar hole in the other end of the case. These terminal boards are made of 1-8 or ¼ inch fibre, mounted in place by two small screws. The binding posts are dry cell binding posts, which may be obtained from some old dry cells at any garage. Referring to Fig. 10, giving a diagram of the taps, the binding posts may be connected in the order given, when the voltages between the taps will be as given.

A one ampere fuse should be placed in the primary circuit.

If it is not desired to mount the transformer in a case, it may be mounted on the bench or on a small switch board by means of the rods holding the clamping strips together. With this arrangement, the connection bindingposts may be mounted on a sheet of fibre fastened to the channel irons.

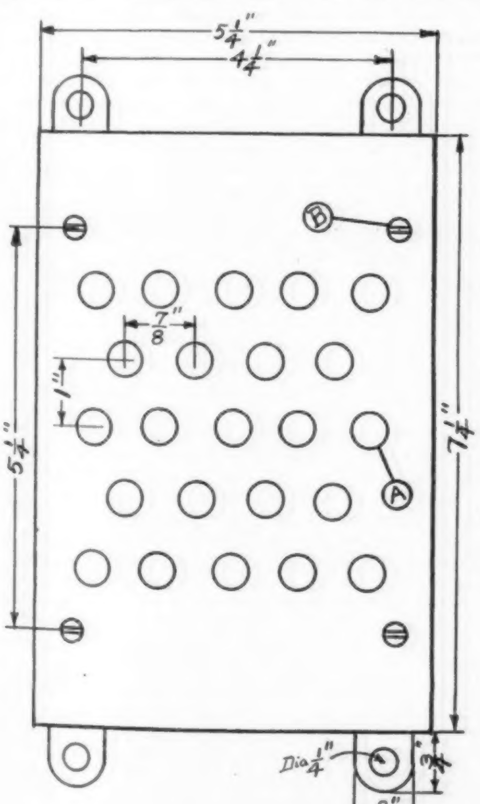


Fig. 11.—Details of containing case.

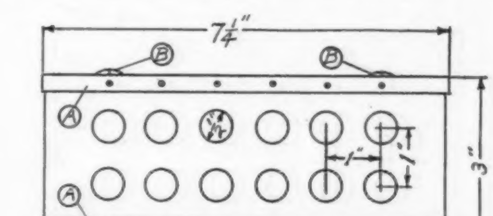


Fig. 12.—Elevation of containing case.

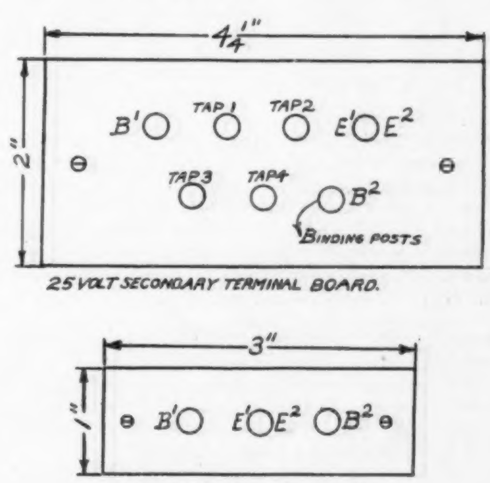


Fig. 13.—The terminal board.

Over-crowded Street Cars

A narrow car; seats filled with persons attempting to read newspapers while the car swings and jolts along its way; aisles jammed with men and women, boys and girls and tiny children, swaying and rubbing, one against the other, coughing and sneezing, pushing and pressing—what a sight for a progressive age; what a sermon for the moralist; what a despair for the student of public health and hygiene! Endless problems are presented by this picture, seen daily in nearly every American city. Most important is the meance to health from the thousands of bacteria, hidden in the throats of diseased men and women, and sprayed directly into a stagnant air, moist and unmoving in the absence of sufficient means of ventilation. Virulent organisms are inhaled into the throats and lungs of tired workers and tiny babes, who form an excellent host for their quick cultivation. The fare for the ride is small, but the cost cannot be estimated in terms of dollars and cents.—*Journal of the American Medical Association.*

Mud-built Farms.—The ingenious practice of "warping," described by J. Stephenson in the *Journal of* the Royal Agricultural Society of England, prevails along a number of rivers flowing into the Humber, in northeastern England. The water carried upstream by the tide is heavily charged with "warp," or mud. By means of artificial channels, provided with sluice gates, the muddy water is carried inland twice a day in summer, at flood tide, and allowed to deposit its sediment over the low-lying swales back of the river banks. In two or three years the surface of these lowlands has been raised a few feet, and the land is made available for agriculture. Flooding is then stopped, the channels being thereafter used as drains for the reclaimed land.

Use of Ice and Other Means of Preserving Food in Homes*

Most Refrigerators in Common Use are Grossly Uneconomical

By John R. Williams, M.D.

The problem of pure milk is only partly solved with the introduction of clean milk into the home. The cleanest and purest milk may soon become foul if adequate means for its preservation are not used. It is well known that bacteria multiply with great rapidity above 50 deg. Fahr., while below that temperature their growth is held markedly in check. These facts are of importance to mothers and housekeepers, also to milk commissions and dealers, because charges of "bad milk" are often unjustly made against those interested in pure-milk work.

In a recent study of the market milk problem in Rochester, it was noted that for purposes of economy nearly half of the families of the city endeavor to get along without ice. In such homes condensed milk, proprietary milk and dipped milk purchased at the near-by store are mainly used. These facts led me to investigate more intimately the use of ice in homes and the means available for the preservation of food. Five sections of the city, representing socially and economically different classes of people, were studied. Upward of a hundred homes in each district were visited. Information was secured, when possible, along the following lines:

1. Inquiry into the use of milk, means of caring for it, etc.
2. Temperature measurements of refrigerators, living rooms and cellars.
3. The make, size and description of refrigerator.
4. The amount of ice used weekly and yearly, the cost of the ice, name of dealer, etc.

During the warm months of the year, milk is delivered during the night or early morning, on an average of three hours before the family is awake. When no box is provided for receiving it, it must be deposited on the door-step where it is exposed to the heat, dust and the attentions of the domestic animals of the neighborhood. Few homes in those sections occupied by working people are equipped with boxes for receiving milk. In an investigation on this point, of 604 homes examined, only 11 had milk boxes. Houses in the well-to-do sections are better equipped, for 359 out of 411 had proper facilities for receiving milk. The exposure of milk to warm air for three hours or more is sufficient to raise its temperature at least 10 degrees.

TABLE I.—USE OF ICE AND MILK AND THE NUMBER OF DEALERS ENGAGED IN THEIR DISTRIBUTION.[1]

Section.	Number Homes Studied.	Number People in Homes.	Number Homes Taking Ice.	Number Ice Companies Supplying Section.	Number Milk Dealers Supplying Section.	Number Homes Having Milk Boxes.
Well-to-do	143	613	134	8	30	91
American laboring	118	509	71	7	27	14
Jewish laboring	155	785	120	17	48	15
German-American laboring	49	210	29	7	22	2
Italian laboring	54	199	4	....	....	2
Totals	519	2,316	358	....	....	124

[1]The foregoing data do not fully show the waste in ice distribution occasioned by the overlapping of routes. There is a different ice company on every street for each five to fifteen consumers. In the first section, on Dartmouth Street, there is a different distributor for each eight consumers. In the Jewish section, on Baden Street, eight ice dealers supply forty-eight homes. In the American laboring section, on Adams Street, there is an ice dealer for each four consumers.

TABLE II.—TEMPERATURES OF REFRIGERATORS, LIVING-ROOMS AND CELLARS DURING MONTH OF AUGUST, 1912, ROCHESTER, N. Y.

Section.	Refrigerators.				Living Rooms.			Cellars.		
	Below 45°	45° to 50°	51° to 60°	Above 60°	Below 60°	60° to 70°	Above 70°	Below 55°	Below 60°	Above 60°
Well-to-do	37	34	48	1	...	64	35	...	6	78
American laboring	1	10	18	10	...	24	11	...	22	29
Jewish laboring	16	14	34	8	...	28	44	...	...	75
German-American laboring	1.	...	19	2	...	4	18	...	4	29
Italian laboring	...	...	...	...	...	...	...	...	...	10
Totals	45	58	119	21	...	120	108	...	32	221

SUMMARY.

Refrigerators studied	243
Refrigerator temperatures below 50 deg. Fahr.	103
Refrigerator temperatures above 50 deg. Fahr.	140
Refrigerators in well-to-do section below 50 deg. Fahr.	61
Refrigerators in well-to-do section above 50 deg. Fahr.	49
Refrigerators in laboring section below 50 deg. Fahr.	43
Refrigerators in laboring section above 50 deg. Fahr.	91
Number of cellars or living-rooms below 55 deg. Fahr.	0
Number of living-rooms examined	228
Number of cellars examined	253

*Read in the Section on Preventive Medicine and Public Health of the American Medical Association, at the sixty-fourth annual session, held at Minneapolis, and published in the *Journal* of the Association.

TABLE III.—THICKNESS OF WALLS OF REFRIGERATORS.[2]

Section.	Less than 2 inches.	2 to 2¼ inches, inclusive	2¼ to 2¾ inches.	3 inches or more.
Well-to-do	5	36	34	19
American laboring	9	23	4	3
Jewish laboring	17	42	8	1
German-American labor'g.	4	13	3	1
Italian laboring	....	....	....	....
Totals	35	114	49	24

[2]A properly constructed box, to be economically operated, should have a wall of efficient insulating material at least 6 inches thick. Such a box at the current price of ice will have a theoretical efficiency of about 80 per cent. The 149 refrigerators whose wall thickness is less than 2¼ inches, even were they made of the best possible construction, could not have an efficiency above 40 per cent. The remaining seventy-three refrigerators with walls averaging less than 3 inches could not have an efficiency above 50 per cent. As a matter of fact, with the shoddy and imperfect insulating materials used, most of the ice-boxes in common use rate far below their theoretical efficiency.

TABLE IV.—NUMBER OF MONTHS ICE IS USED DURING YEAR BY HOMES IN VARIOUS SECTIONS OF ROCHESTER.[3]

Section.	1 month.	2 months.	3 months.	4 months.	5 months	6 months.	7 months.	8 months.	9 to 12 mos.	Total.
Well-to-do	...	2	5	15	22	24	15	8	33	124
American laboring	1	6	10	14	21	3	4	4	3	66
Jewish laboring	...	4	16	26	47	15	5	1	1	115
German-American laboring	1	1	4	8	15	...	...	...	1	30
Italian laboring	...	...	2	...	...	...	...	...	...	2
Totals	2	13	37	63	105	42	24	13	38	337

[3]This table shows, among other things, the seasonal character of the use of ice. This adds greatly to the cost of distribution, because it necessitates a large investment in equipment, most of which is idle during half the year.

The usual means of preserving perishable fresh foods in the home is to store them in a cool place as the pantry, cellar or refrigerator. When the last is not afforded, either the cellar or living-room is used. In a former and more extended inquiry into the use of ice in all parts of the city, it was learned that 2,243 homes of 5,431 examined did not use ice. In the present and more limited investigation, in which the very rich and very poor were largely avoided, 161 homes of 519 studied were not using ice any of the time. It is probably within the truth to say that half of the homes in the city rely the entire year on the cellar or pantry for their food preservation, and that more than three quarters of the homes deny themselves the use of ice, excepting for a few weeks during midsummer.

In view of these facts temperature observations of cellars and living-rooms where food is stored as well as refrigerators, are of importance. In Table 2 it will be seen that not one living-room was found having a temperature below 60 deg. Fahr., nor was one cellar discovered having a temperature below 55 deg. Fahr. It is clearly evident therefore that these rooms during the warm months of the year, at least, are not sufficiently cold to protect food from bacterial decomposition.

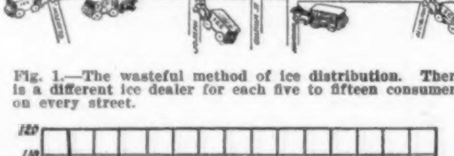


Fig. 1.—The wasteful method of ice distribution. There is a different ice dealer for each five to fifteen consumers on every street.

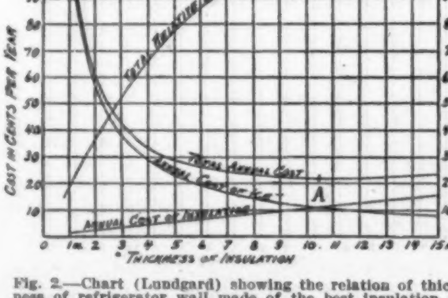


Fig. 2.—Chart (Lundgard) showing the relation of thickness of refrigerator wall made of the best insulation to the efficiency of the refrigerator, and also the comparative cost for operation per square foot of wall surface.

In the study of ice-boxes, 243 were examined. Of these only 103 had a temperature below 50 deg. Fahr. The other 143 registered above that temperature and were therefore worthless for preserving food. As would be expected, a better class of refrigerators is to be found in the homes of the well-to-do; nevertheless, 45 per cent of these had temperatures above 50 deg. Fahr., while nearly 70 per cent of those found in the homes of the working people exceeded that temperature.

The main reason for the inefficiency of these refrigerators is to be found in their defective construction and insufficient insulation. Most of them are wooden boxes built of half-inch lumber, and are lined with galvanized iron or zinc. The walls vary in thickness from less than 2 inches to less than 4 inches (see Table 3). The space between the metal lining and the wooden sides usually contains insulating material, as paper, felt or mineral wool. In many of them nothing more is to be found than a sheet or two of paper. Since the efficiency of a refrigerator depends in large part on the character and thickness of the insulating material, consideration must be given to these factors. It has been proved both experimentally and practically that confined air or a partial vacuum are the best insulators. Indeed, it is the air entrapped within its cellular structure which retards the passage of heat through a material and gives it insulating value. The air must be confined, for circulating air is a good medium for the conduction of heat. A good insulator therefore must have a physical structure which will hold air within its cells or between its fibers. Moreover, it must be water-proof. Moisture, because of its greater power of heat conductivity, is a fatal enemy to perfect insulation.

A refrigerator wall which contains a space large enough to permit of air circulation will be found defective. Wood, felt, mineral wool, charcoal, sawdust, wood-ashes, etc., are fairly efficient when they are dry, but when damp or wet their efficiency markedly declines. When an efficient insulator is used there is very little difference between the temperature of the inside of the box and the temperature of that side of the metal lining which is next to the insulating material, consequently there will be very little deposition of moisture on the metal lining to be absorbed by the insulator. When the insulator is inefficient, however, the temperature difference is considerable and there is marked condensation of moisture on both sides of the metal lining. If this moisture is absorbed by the insulator it materially lessens its efficiency. In any event it causes the metal lining to corrode and makes the box damp and insanitary. Most of the commonly used insulators take up moisture with avidity, and for this reason most of the cheap refrigerators in common use will be found coated on the inner side of the inside wall with slime and the mineral salts resulting from the metallic corrosion.

Properly made cork board is probably the best-known and most efficient insulating agent. It is made of ground cork pressed into slabs and heated until the resins in the cork cement its particles together. It is to the remarkable physical structure of cork that its insulating efficiency is due. It consists of a woody

TABLE V.—NUMBER OF HOMES USING VARIOUS AMOUNTS OF ICE WEEKLY.

Section.	50 pounds or less.	75 pounds.	100 pounds.	100 to 200 pounds.	200 to 300 pounds.	300 pounds, Plus.	Total.
Well-to-do	....	3	22	74	24	11	134
American laboring	11	15	18	23	2	1	70
Jewish laboring	5	18	8	16	2	....	49
German-American laboring	3	5	8	15	....	....	31
Italian laboring	4	....	....	....	....	....	4
Totals	23	41	56	128	28	12	288

TABLE VI.—PRICE PAID FOR ICE PER YEAR; DATA FROM 321 FAMILIES.[4]

Section.	Under $5.	$5 to $10.	$10 to $15.	$15 to $20.	$20 and over
Well-to-do	6	36	33	13	34
American laboring	34	16	5	1	4
Jewish laboring	22	72	10	6	1
German-American laboring	8	14	1	1	....
Italian laboring	4	....	....	....	....
Totals	74	138	49	21	39

[4]Two hundred and twelve families pay $10 or less per year; 109 families pay more than $10 per year. It is worthy of note that at least 75 per cent of the money each family pays for ice is wasted, partly in the uneconomical method of distribution, but chiefly in the inefficiency of the refrigerators.

material, filled with tiny air cells through which air circulation is not possible; moreover, it is impervious to moisture.

The comparative value of dry wood and the best cork board is as 1 is to 5; that is to say, 1 inch of cork board is equivalent in insulating value to 5 inches of wood. Since it requires 6 inches of cork board to give approximately 80 per cent of efficiency in cooling, at the current prices of ice, it follows that few refrigerators in common use have an efficiency above 25 per cent. Indeed, the low-priced boxes used in the homes of working people are probably less than 15 per cent efficient. This means that of 100 pounds of ice put into the refrigerator, at least 80 pounds are used in neutralizing the heat which percolates through the wall. It is worthy of note that the market is flooded with these shoddy ice-boxes. No less than seventy-five different makes were found among the 243 examined. Few of them bear the names of their makers, which suggests that the manufacturer is not proud of his handiwork.

The rate paid by the consumer for ice varies according to the amount used, the amount taken at each delivery, and the ability of the purchaser to pay for it in advance. Poor people living near the railway tracks get it at the cars for 20 cents per hundred pounds. If it is delivered to their homes they pay from 40 to 75 cents per hundred pounds.

The cost of harvesting and storing natural ice is so variable that it is difficult to determine it even approximately. It is probably within the truth to say that it costs less than $2 per ton. It costs between $2 and $2.25 per ton to make artificial distilled-water ice, although by some of the newer methods this cost has been materially reduced. The consumer pays the dealer the difference between $2.25, the cost of manufacture, and $8.50 per ton, or $6.25 for distribution. Thus, the cost of distribution is nearly three times the cost of manufacture. One reason for this is the wasteful method employed. In Rochester a different ice company will be found supplying each five to fifteen consumers on every street, necessitating a tremendous waste in labor and delivery equipment.

CONCLUSIONS.

The data gathered in this investigation warrant the following conclusions:

1. The temperatures of cellars or living-rooms in dwelling-houses are not sufficiently low during the warm months of the year to protect milk and other perishable foods from rapid bacterial decomposition. Therefore an efficient refrigerator in the home is a necessity.

2. Most of the refrigerators in common use are almost worthless and grossly uneconomical.

3. There is a large field for the manufacturer who will make a properly insulated and efficient box which can be sold at a moderate price.

4. If more economical methods of ice manufacture and distribution were employed, the cost of ice to the consumer could be materially lowered.

5. If to this saving were added that which would result from proper ice-box construction, refrigeration vastly superior to that now found in the average home could be had for at least one fourth the present cost.

Economy in Ocean Transport*

In his James Forrest lecture at the Institution of Civil Engineers, Mr. Alexander Gracie achieved the success of many exponents of scientific subjects, in that he disclosed so much as to justify a general desire for more. Although his subject was "twenty years' progress in marine construction," nothing was said of the Navy, notwithstanding that there has been greater progress in respect of propulsive and thermodynamical efficiency in this department of marine construction than in connection with mercantile ships. We understand, however, that the lecturer was requested to deal only with the mercantile marine. This is the more regrettable, since Mr. Gracie is not only chairman of one of the most prominent warship-building companies, but has been a member of Admiralty Committees on design problems, and is therefore specially able to deal with recent progress in both the Royal and merchant navies. Nevertheless, the lecture contained much that justified congratulation on the part of those engaged in design and constructional work for ocean transport, and at the same time tended to stimulate reflection. We are glad that Mr. Gracie entered fully into the subject as it affects the humbler, although very numerous, units of the world's over-sea carriers, the intermediate liner and cargo tramp; because progress with such vessels is seldom, if ever, analysed. And yet, as a nation, we are largely dependent on ocean transport for food supplies and raw materials for many industries. Thus the influences of economy in cargo-carrying vessels is far-reaching, and the incentive to improvement most pronounced.

Quite 80 per cent of the world's immense fleet of merchant ships may be classed as cargo-carriers, although many take a few passengers. The pinch of Parliamentary provisions is year by year more keenly felt. Load-line conditions, constructional requirements, lifeboat rules, and food and other regulations, along with the demands of labor and port authorities, tend to increase expense. Reason, to a great extent, and sentiment, to a less degree, support such regulations as are made. On the other hand, competition leads to a reduction in the profits of shipowning; for, although lately there has been a period of high freights, the general trend is downward. The shipowner, therefore, looks to the naval constructor to evolve means whereby the cost of propulsion can be reduced, for his interest is along the road toward improvement, as the rate of obsolescence of cargo ships has a direct effect on the demand for new tonnage, on the principle of the survival of the fittest. It becomes interesting, therefore, to analyze the past with a view to discover new lines of advance for the future. Mr. Gracie showed the effect of increased size on the economy of ocean transport. Assuming a 3,000-mile voyage at 13 knots speed, a ship having capacity for 4,000 tons of cargo would consume 12¾ tons of coal per 100 tons of cargo on the trip, while a ship carrying 8,700 tons of cargo would only require 8 tons. Thus the larger ship shows a saving of 36 per cent in the fuel bill, which, with the price of coal increasing steadily, is a considerable gain in a yearly balance-sheet. Again, the constructional material in the larger ship is only 77¼ tons per 100 tons of cargo capacity, against 92½ tons, a 16 per cent gain upon the first cost, interest, and depreciation. Is it surprising that there has been a steady growth in the size of cargo tramps? In one typical line the growth in eighteen years is shown to be from 6,400 tons to 9,600 tons dead-weight carrying capacity. Less power and, therefore, less fuel consumption is required for a given speed with the larger ship. For 11 knots on a 3,000-mile voyage 1 ton of coal carries 23.5 tons of cargo in the smaller, and 26.4 tons in the larger, ship for the full 3,000 miles.

The higher efficiency of present-day machinery is an important factor in the economy of ocean transport. Higher steam pressures and multiple-compound engines have had their effect. The rate of consumption of coal has in twenty years been decreased from 1.6 pounds to 1.3 pounds per horse-power per hour; but the limit has been reached with reciprocating engines, as it is doubtful if superheating the steam will be widely accepted in the tramp ship, because of complications or of difficulties with lubrication and with oil passing over to the boiler and superheater. There are far greater prospects for the turbine working the propeller through helical gearing. When well designed and accurately made with sound material the turbines and the gear, operated with a satisfactory lubricating system, require less attention, and, for that matter, less intelligence on the part of the watch-keeping engineer than the reciprocating engine; so that although the first cost may at present not be on the side of the former, the gain in economy in the long run must be in favor of the geared turbine. Mr. Gracie shows that in coal consumption there is a gain of 24 per cent in a single-screw geared-turbine tramp ship over the old single-screw triple-expansion-engined ship, while the weight of machinery is 15½ per cent less. In the case of two new vessels, "Cairnross" and "Cairngowan," of about 9,900 tons displacement, identical in all respects, except that one was fitted with geared turbines and the other with the latest type of triple-expansion engines, the results of voyages simultaneously made in the same seas was an economy of 15 per cent in favor of the geared turbine. There is economy, too, in respect of the lubrication and of wages, as the staff is reduced. Herein lies a possible source of improved balance-sheets of tramp steamers. Mr. Gracie also referred to improvements in cargo-handling gear, and to the advantages of larger hatches in reducing the extent of movement of cargo in the holds when being discharged, but it is difficult in such case to give a definite figure of gain in economy.

Many data were given by Mr. Gracie as to improvements in other types of ships in the direction of comfort, speed, and economy. In Atlantic liners the average state room area per passenger has increased in twenty years from 17¾ square feet to 80 square feet, and the average area of public rooms from 16 square feet to 40 square feet, and the trial speed from 22 to 26 knots, the girder ratio, the ratio of length to structural depth, has decreased from 14.45 to 12.5, while the fuel consumption per unit of power is 10 per cent less, and the weight of machinery 40 per cent less. In Channel steamers equally great advance has been made. The speed has advanced from 20.64 knots to 25.07 knots, the speed-length ratio (the speed divided by the square root of the length of hull) going up from 1.15 to 1.47, a result only surpassed by torpedo craft. The fuel consumption in the twenty years has decreased 24 per cent, and the weight of machinery 40 per cent. The present-day results for Channel steamers were got with geared turbines and watertube boilers in the Newhaven and Dieppe steamer "Paris."

* *Engineering.*

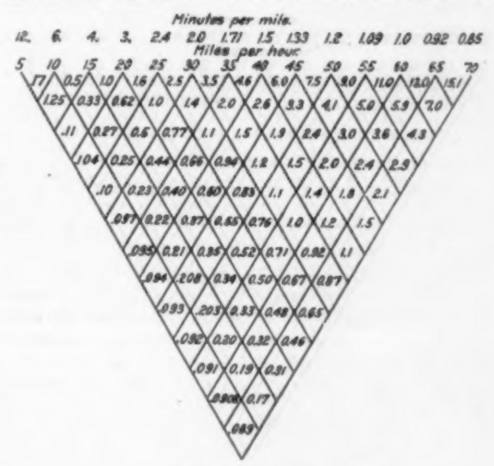
Rate at Which Time of Trains Can Be Made Up

By Paul M. La Bach

In riding on trains which are late we frequently hear discussions regarding making up time. The ordinary passenger, at least, thinks this can be very readily done if any one desires to do so. Whether time can be made up depends, of course, upon the schedule and the ability of the locomotive to make any increases in speed.

Just what happens when a train makes up a minute is shown in the accompanying table. The dividing lines indicate the speeds shown at their upper ends. The figures in the acute angles between these lines show the distance in miles the train must travel in order to make up one minute. Thus, if the train is running at an average speed of 35 miles an hour and increases it to 40 it will make up a minute in 4.6 miles. If it increases it from 35 to 45 miles an hour a minute will be made up in 2.6 miles. If the speed increases from 10 to 70 miles an hour, a minute will be made up in 0.17 miles.

A table of this kind may be used in making up tentative schedules. For instance, if two points are 46 miles apart and the average speed is 35 m. p. h., what would be the



A Speed Diagram.

saving in time by increasing the speed to 40 miles? Increasing speed 35 to 40 miles gives 4.6 miles as the distance required to save one minute. This divided into 46 miles gives 10 minutes.

The diagram may be used to find out the speed when the amount of time made up is known. A train with a scheduled speed between two points 50 miles apart of 40 miles an hour makes up 25 minutes. How fast did it go? 50 ÷ 25 = 2 miles in which it makes up 1 minute. Running down the 40 line we come to 2.0 at the intersection with the 60 m. p. h. line. The answer is that an average speed of 60 m. p. h. was made. It is apparent from a study of a table made up on this basis that a greater distance is required to make up a given amount of time than a layman would believe.—*Railway Age Gazette.*

Electric Light and Eyesight

From the beginning repeated attempts have been made to create scares on the subject of eyesight injury through electric lighting; and thus the allegation is not surprising that there is something mischievous in the rays of the tungsten lamp. The mere utterance of the word "ultra-violet" seems to have some curious psychological influence. Yet its application to the wire filament means no more than that this method of artificial illumination possesses in this respect at least the quality of sunshine. What is undoubtedly injurious to the eyes, whether it is experienced in the full flood of daylight or under electric, gas, or oil lamps at night, is glare; and it is the work of the illuminating specialist to give us light without this glare. His task is made the easier by the amiable servility with which the tungsten lamp submits to control in regard to its position and its shading; and it is an interesting feature of the trade that the development of the tungsten lamp business in the past two or three years has been accompanied by the production of a great variety of beautiful and ingenious fittings and apparatus for enabling the public to take full advantage of the wire filament's economy and power. Artificial lighting is becoming more grateful to the eye every year.—*London Times.*

Hardening and Tempering Steel*

A Review of the General Requirements and Characteristics of Quenching and Tempering Baths

WHILE there have been many articles published regarding the hardening and tempering of steel and the furnaces used, there is but little detailed information available regarding the baths used for these operations. The time has long since passed when each hardener had his own carefully guarded secrets regarding the composition of quenching baths. On the other hand, the time has also passed when it was a common belief that to harden a piece of steel it was only necessary to cool it off more or less rapidly in almost any kind of cooling medium; it has been found that the cooling mediums for hardening and the heating mediums or baths for tempering do, after all, play quite an important part both as regards economy and the efficiency of the tools treated. It is the intention in this article to outline the methods which have proved most successful, and to give the compositions of baths which from long experience in connection with hardening operations have proved to give the best all around results and to be the most economical; in many cases they have not been the cheapest in initial cost, but nevertheless are most economical because of the better results obtained with the tools treated and the greater length of time that the baths could be used before deteriorating. A description of such receptacles—cooling tanks and tempering furnaces—for the treatment of steel as have proved to be the best for all around purposes has also been included.

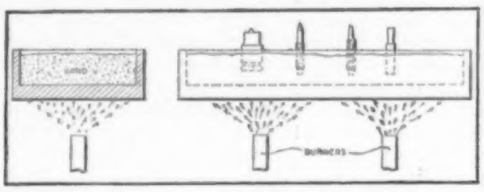


Fig. 1.—Arrangement used for sand tempering. Graded tempering can be given in this bath.

For the sake of convenience the subject will be divided into four distinct parts as follows: (1) Baths used for cooling (quenching). (2) Baths used for tempering (drawing the temper after hardening). (3) Some tests and analysis of baths (1) and (2). (4) Receptacles and furnaces used in quenching and tempering.

CHARACTERISTICS OF QUENCHING BATHS.

No matter what the composition of a quenching bath, to insure uniform hardening the temperature of the bath must be kept constant, so that successive pieces of steel or tools quenched will be acted upon by baths of the same heat. The necessity of a uniform temperature for a quenching bath will be readily understood by reference to ordinary water for a cooling bath; everyone having any knowledge of the subject knows that a tool quenched in such a bath at room temperature will come out much harder than if quenched when the water is at the boiling point. In fact, it is well known that one way of partially annealing steel is by plunging it at a red heat into hot water. The same difference in hardness will result when using any quenching bath at different temperatures, and hence no actual and dependable data can be obtained unless means are taken for keeping these baths at a uniform heat.

When using quenching baths of different composition the tools quenched will vary in hardness. This is due mainly to the difference in heat-dissipating power of the different baths. Thus a tool hardened at the same temperature in water and brine will come out harder when quenched in brine; the greater the conductivity of the bath the quicker the cooling. The general opinion, to-day, is that the composition of a quenching bath is of small importance as long as the bath cools the pieces rapidly. Those who have made a study of the subject have found different opinions regarding the same quenching bath by different users, and a good many quenching fluids have been condemned owing to improper heating and in many cases to improperly built furnaces. As an example may be cited an oven furnace with which the user once had trouble. Owing to faulty construction of this furnace, more air was let into the heating chamber of the furnace than could be taken care of by the fuel oil; after having condemned first the steel and then the quenching bath, and then trying one quenching bath after another with the same results, it was suggested that the "heating" did not look just right, and an expert was called in to find out what the trouble was. After much experimenting with the burners and the furnace itself good results were finally obtained. The difficulty seemed to be that the oxygen of the air attacked the steel and formed oxide of iron on the surface of the tools, which consequently had a soft scale on the outside.

Those who are skeptical as to there being any difference in the effect on steel of cooling baths of different composition will readily admit that it is advantageous to use baths free from oxygen and from ingredients that tend to oxidize. Quenching baths should be uniform; good tool steels of high carbon are very sensitive to differences in both water and oils. Water for hardening tool steel should be soft; entirely different and very unsatisfactory results will be obtained when using hard water. While different quenching oils show less difference in the results obtained, vegetable and animal oils will give somewhat different degrees of hardness depending upon the sources from which they are obtained. One cannot be too careful in the selection of water, as it is likely to contain many impurities. If it contains greasy matters, it may not harden steel at all, whereas if it contains certain acids, it will be likely to make the tools quenched in it brittle and even crack them.

LIST OF QUENCHING BATHS.

(1) Water—soft—preferably distilled; good tool steel should require no mixture added to pure water. (2) Salt added to water will produce a harder "scale" than if quenched in plain water. (3) Sea (salt) water—the keenest natural water for hardening. (4) Water as under (1), containing soap. (5) Sweet milk.[1] (6) Mercury.[1] (7) Carbonate of lime.[1] (8) Wax.[1] (9) Tallow.[1] (10) Air—mostly used for high-speed steel; mere exposure, however, is in many cases and on many steels not sufficient to produce hardness and an air blast is necessary, as this furnishes cool air in rapid motion. (11) Oils[2] such as cottonseed, linseed, whale, fish, lard, lard and paraffine mixed, special quenching oils, etc.

The order of the intensity with which various cooling baths will harden steel of about 0.90 to 1.00 per cent carbon is as follows: Mercury, carbonate of lime, pure water, water containing soap, sweet milk, different kinds of oils, tallow and wax. In all cases, except possibly the oils, tallow and wax, it must be remembered that the tools become harder as the temperature of the bath becomes lower.

[1] Generally used for special purposes only.

[2] A small quantity of sal-ammoniac added to the oil bath has a tendency to make the tools come out clean from the bath.

* Reproduced from *Machinery*.

BATHS USED FOR TEMPERING.

The object of tempering is to reduce the hardness and to remove internal strains caused by sudden cooling in quenching. The composition of a tempering bath is of little importance compared with that of a quenching bath when considering the effect upon the pieces treated. Aside from the operator's convenience and possible bad effects upon his health, the different baths used for this operation must be considered with regard to initial cost, lasting quality, effects on finish, etc.

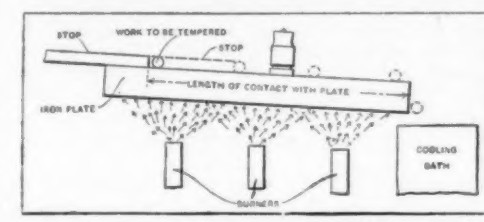


Fig. 2.—Tempering arrangement utilizing an inclined plate on which the objects roll down.

While oil is the most widely used medium for tempering tools in quantities, other means and methods are employed, especially by those who have tools in small quantities to temper, when the expense of installing and running an oil tempering furnace would not be warranted. Of these methods we first find the one used by the old-style tool hardener of only partly cooling the tool when quenching it, then quickly withdrawing it, polishing off the working surface, and then letting the heat which remains in the tool produce the required temper as judged by the color. If the tool has a shank, it is good practice to heat part of the shank also and quench the working part of the tool only, in which case this part can be cooled off thoroughly; the heat remaining in the body or shank of the tool will do the tempering, which also in this case must be judged by the color.

The sand bath is another frequently used medium for tempering, the sand being deposited on an iron plate and heated; by the use of this method a piece to be tempered can be given different tempers throughout its length, as, for example, rivet hole punches; these are placed endwise—bottom down—in the sand about two thirds projecting outside the sand into the air (see Fig. 1). It is readily seen that the nearer the bottom of the sand bath, the higher the heat, and the punch so placed, when tempered right, will have the bottom soft—a deep dark blue—the neck a very dark straw, and the working part of the punch on top a light straw color; thus there is a gradual increase in hardness from the bottom up. Pieces so drawn must previously have been polished, and the temper is judged by the color. When the pieces have attained the right color they are, of course, cooled off, generally in water or oil. A plate without sand similarly heated can also be used, but it is not as satisfactory.

A plate arranged as shown in Fig. 2 will be found very convenient when drawing small, round pieces. The pieces are rolled on the inclined plate which is heated as indicated. The length of time the work is in contact with the plate can be regulated by adjusting the amount of the incline, as well as the location of the "stop." This arrangement can also be used for such work as punches, etc., in which case the plate, of course, should stand level and not in an inclined position.

Another frequently used tempering medium is hot air, the temper in this case also being judged by the color. For this method of tempering special furnaces should be employed in order to get uniform results. This method is used more especially for small and light work in quantities and where the color has to be bright and clear. While all of these methods have the advantage of enabling one to actually see the temper given to tools treated, the oil tempering bath is the one mostly used owing to its economy.

The two main points to be considered when using an oil tempering furnace are: first, to have the heat uniform throughout (not hotter where the burners or flames are in contact with the walls of the furnace); and second, to leave the pieces to be tempered in the oil long enough to have attained the heat of the oil throughout when taken out. The first point can be taken care of, as far as possible, by proper construction of the furnace; the second can best be taken care of by immersing the pieces to be tempered in the oil before starting to heat, and letting the pieces remain in the oil and be heated with it to the temperature required. In such a case, one should, of course, have more than one furnace, or else

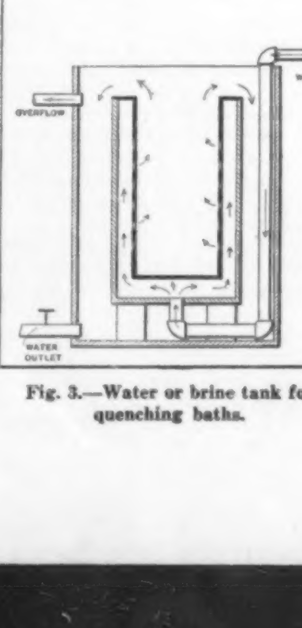


Fig. 3.—Water or brine tank for quenching baths.

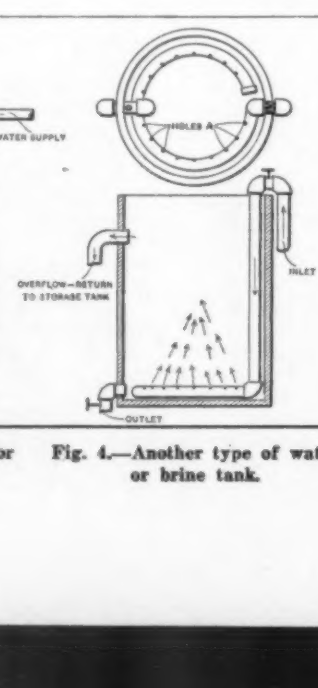


Fig. 4.—Another type of water or brine tank.

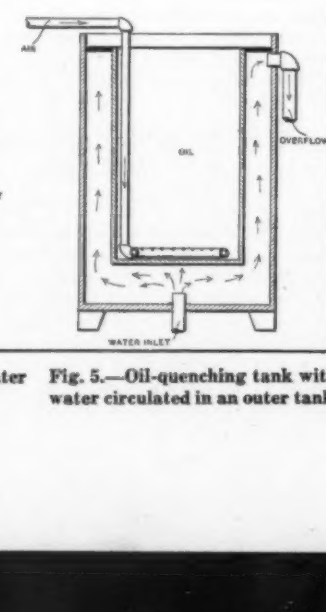


Fig. 5.—Oil-quenching tank with water circulated in an outer tank.

after each operation take the hot oil out and refill the tank with cold. The method described is very much better than the one frequently used of immersing the pieces in a bath which already has the required temperature and then letting them remain long enough to attain the heat of the bath throughout, as a furnace yet has to be designed which will maintain a uniform heat for even as short a time as is required for this operation. Furthermore, it is not necessary that a piece to be tempered be held in the bath a certain length of time at the required temperature; the temperature desired need only be maintained long enough to insure that the piece has been evenly heated throughout.

When tempering to high heats, or, rather, when tempering to higher heats than the flash point of any tempering oils (650 to 700 deg. Fahr.[3]) some other tempering fluid than oil must be used. Lead is the one usually employed. As it is impossible when using lead to let the pieces to be tempered be heated up with the lead, they must be immersed at the predetermined temperature and kept there until heated evenly throughout to the same temperature as the lead. It is claimed by many that it is easier to maintain a uniform heat in a lead bath than in an oil bath, but it has been found that, owing to the lead not circulating as readily, the temperature may vary considerably in different parts of the bath, and hence it is not very reliable.

Salt is another medium frequently employed for tempering heats between 575 and 875 deg. Fahr. Salt fuses at 575 deg. Fahr., but when immersing the pieces to be tempered the salt will immediately solidify around the cold pieces. When these are heated to 575 deg., the salt will melt and the pieces should be withdrawn. This is not reliable, however, as the pieces, especially if large, will not have had time to be heated through before the salt melts. If a higher temper is required, it is, of course, only necessary to let the pieces remain in the bath and get the readings of the heat from a pyrometer. In all these methods, it is questionable if it is good practice to suddenly immerse cold pieces to be drawn into baths of such high temperatures. When a lower temper is required, and an oil tempering bath or furnace is not available, alloys of lead and tin can be used for as low heats as 400 deg. and of lead and antimony for 500 deg. However, this involves the inconvenience of keeping a large number of different alloys on hand, if it is desired to vary the temper heats. The following table for different alloys which melt at the temperatures given was compiled by Mr. O. M. Becker.

Lead.	Tin.	Melting Temerature, Deg. Fahr.	Lead.	Tin.	Melting Temperature, Deg. Fahr.
14	8	420	24	8	480
15	8	430	28	8	490
16	8	440	38	8	510
17	8	450	60	8	530
18.5	8	460	96	8	550
20	8	470	200	8	560

TESTS AND ANALYSIS.

The analysis and test results of oils when new (not used as compared with those of oils which have been used such a length of time as to render them practically valueless will be found interesting.

	Tempering Oil.		Lard and Paraffine oil mixed (half and half) used for quenching.	
	New.	Old (thick).	New.	Old (thick).
Flash point	550	475	400	380
Fire test	625	550	475	450
Mineral oil, per cent	94	30	25	10
Saponifiable oil, per cent	6	70	75	90
Specific gravity	0.920	0.950	0.912	0.925

The great difference in tests and analysis between new and used oils should be noted; oils used constantly at high heats will gradually lose the "mineral" part of the oil, the more so the higher the heat used. A tempering bath can therefore be prolonged in life by adding to it now and then new mineral oil. To lengthen the life of the bath high heats should be avoided as much as possible.

[3] There are tempering oils on the market claimed to have a flash test of 750 degrees, but it is doubtful if they ever have been found to stand this test. Heavy black cylinder oil has been found to stand a flash test of 725 degrees.

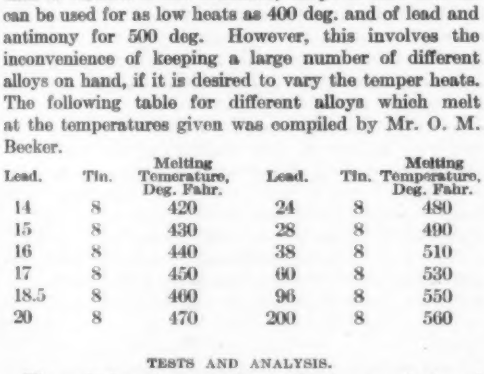


Fig. 9.—**Ordinary type of tempering furnace.**

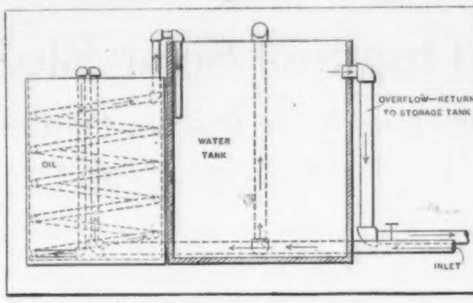


Fig. 6.—Water and oil tank combined.

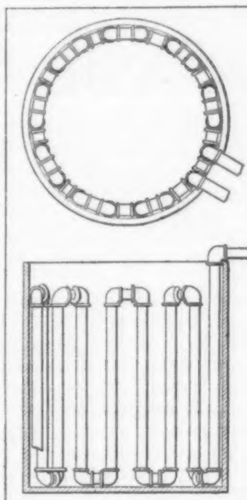


Fig. 7.—Ordinary type of quenching tank.

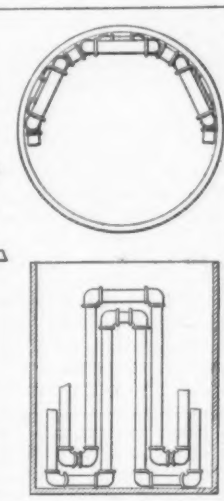


Fig. 8.—Oil-quenching tank with water and steam coils.

RECEPTACLES AND FURNACES USED IN QUENCHING AND TEMPERING.

The main point to be considered in a quenching bath is, as mentioned, to keep it at a uniform temperature so that successive pieces quenched will be subjected to the same heat. The next consideration is to keep the bath agitated, so that it will not be of different temperatures in different places; if thoroughly agitated and kept in motion, as is the case with the bath shown in Fig. 3, it is not even necessary to keep the pieces in motion in the bath, as steam will not be likely to form around the pieces quenched. Experience has proved that if a piece is held still in a thoroughly agitated bath, it will come out much straighter than if it has been moved around in an unagitated bath. This is an important consideration, especially when hardening long pieces. It is, besides, no easy matter to keep heavy and long pieces in motion unless it be done by mechanical means.

In Fig. 3 is shown a water or brine tank for quenching baths. Water is forced by a pump or other means through the supply pipe into the intermediate space between the outer and inner tank. From the intermediate space it is forced into the inner tank through holes as indicated. The water returns to the storage tank by overflowing from the inner tank into the outer one and then through the overflow pipe as indicated. In Fig. 4 is shown another water or brine tank of a more common type. In this case the water or brine is pumped from the storage tank and continuously returned to it. If the storage tank contains a large volume of water, there is no need of a special means for cooling. Otherwise, arrangements must be made for cooling the water after it has passed through the tank. The bath is agitated by the force with which the water is pumped into it. The holes at *A* are drilled on an angle, so as to throw the water toward the center of the tank. In Fig. 3 is shown an oil quenching tank in which water is circulated in an outer surrounding tank for keeping the oil bath cool. Air is forced into the oil bath to keep it agitated.

Fig. 6 shows a water and oil tank combined. The oil is kept cool by a coil passing through it in which water is circulated, which later passes into the water tank. The water and oil bath in this case is not agitated.

Fig. 7 shows the ordinary type of quenching tank cooled by water forced through a coil of pipe. This can be used for either oil, water or brine. Fig. 8 shows a similar type of quenching tank, but with two coils of pipe. Water flows through one of these and steam through the other. By this means it is possible to keep the bath at a constant temperature.

TEMPERING FURNACES.

In tempering furnaces the only really important consideration is to insure that the furnace is so built as to heat the bath uniformly throughout. It is doubtful if there can be found a tempering furnace on the market that will fill this requirement entirely, although many give good results in general. It is never safe, however, to let any tools being tempered rest against the bottom or sides of the tank, as no matter how scientifically the furnace may be built these parts are, in most cases, hotter than the fluid itself. It is, of course, just as important not to let the thermometer rest against any of these parts in order to insure correct readings. After the pieces tempered are taken out of the oil bath, they should immediately be dipped in a tank of caustic soda (not registering over 8 or 9), and after that in a tank of hot water. This will remove all oil which might adhere to the tools.

Fig. 9 shows an ordinary type of tempering furnace. In this the flame does not strike the walls of the tank directly. The tools to be tempered are laid in a basket which is immersed in the oil. In Fig. 10 is shown a tempering furnace in which means are provided for preventing the tools to be tempered from coming in contact with the walls or bottom of the furnace proper. The basket holding the tools is immersed in the inner perforated oil tank. This same arrangement can, of course, be applied to the furnace shown in Fig. 9.

Natural Gaseous Mixtures Rich in Helium

The following mineral springs evolve gas containing a large percentage of helium. The natural gas from the springs at Sautenay contains from 8.4 to 10.1 per cent of helium, that from springs at Maizières contains 5.9 per cent; from springs at Grisy, 2.18 per cent; from Bourbon Lancy, 1.84 per cent; from Néris, 0.97 per cent; and from La Bourboule, only 0.1 per cent. The quantities are too large to be considered as nascent helium evolved immediately it is produced; and it is more probable that the immediate source is dissolved helium, evolved by the disintegration of minerals in which it has been absorbed. These sources are grouped in the neighborhood of Moulins, Dijon, and Vesoul.—*Journal of the Franklin Institute.*

Development of Water Power in Nova Scotia

Nova Scotia has many rivers, with capacious lakes for storing purposes, and with falls from 11 feet to 100 feet, where from 100 to 30,000 horse-power could be developed at a very reasonable cost. The River Mersey is one of the largest rivers in Nova Scotia. It has a fall of 248 feet from First Lake to tide water, a distance of 17 miles. Four power developments already exist on the river, occupying nearly 6 miles of its lower reaches. The whole river has lately been surveyed, and it is proposed to raise the level of First Lake by 20 feet, to provide storage for future developments. Three additional dams are to be built, which will transform the river into a series of mill ponds.—*Engineering.*

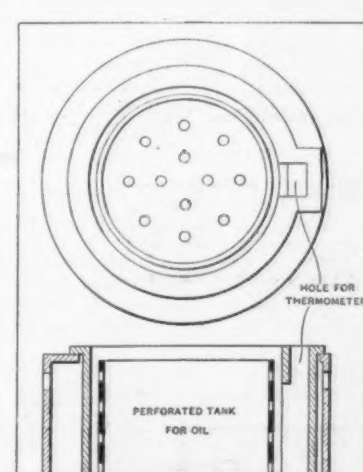


Fig. 10.—Special tempering furnace with perforated oil tank.

On the Origin of Solar Electricity*

What Evidence Have We of Charged Particles in the Sun?

By J. A. Harker, D Sc , F.R S.

MORE than twenty years ago, in his presidential address to the Physics Section of the British Association at Edinburgh, when discussing solar phenomena, Professor Schuster (*Brit. Assoc. Report*, 1892, p. 635) said:

"May not the periodicity of sun-spots and the connection between two such dissimilar phenomena as spots on the sun and magnetic disturbances on the earth be due to a periodically recurring increase in the electric conductivity of the parts of space surrounding the sun?"

Professor Hale (*Proc. Roy Inst.*, xix., 621), in his Royal Institution lecture in 1909 on "Solar Vortices and Magnetic Fields," described the methods by which he had detected within the vortices, shown by the spectroheliograph to be present in sun-spots, magnetic fields of considerable strength. In discussing the probable origin of these fields, Hale says:

"We know from Rowland's experiment that the rapid revolution of an electrically charged body will produce a magnetic field. Thus if a sufficient number of electrically charged particles were set into rapid revolution by the solar vortices, a magnetic field should result. What warrant have we for assuming the existence of charged particles in the sun?"

Some experiments made at the National Physical Laboratory during the past two years by Dr. G. W. C. Kaye[1] and the writer on the emission of electricity from hot bodies seem to have a distinct bearing on these problems. The application to cosmical phenomena of some deductions from these and a few hitherto unpublished further experiments form the subject of the present paper.

It may be well briefly to recapitulate here the results attained. The first experiments dealt with the electric conductivity of the space inside a carbon-tube resistance furnace heated by alternating current. A pair of small insulated exploring electrodes of carbon projected into the furnace at opposite ends, and were connected with a battery of a few volts and a suitable current-measurer. It was found that, as the furnace was gradually heated from room-temperature, the resistance of the interior space remained practically infinite until a temperature of about 1,400 deg. Cent. was reached, when the resistance commenced to fall rapidly. The curve showing the current between the two exploring electrodes was exponential in character.

It was further found that, on removing the battery, and therefore in complete absence of all applied potential, a transient electric current could be obtained by keeping one of the electrodes in a fixed position in the furnace, and heating or cooling the other by moving it in or out of the hot region. The direction of the current indicated a negative[2] emission from the hotter to the colder surface across the gap.[3]

Ionization currents up to about 3 amperes at over 3 volts were thus obtained from a quite small apparatus, having a working electrode-area of only a few square centimeters. If both electrodes are kept stationary in the furnace, and one is maintained permanently hot, and the other, by means of water-cooling, kept at a much lower temperature, continuous currents of the order of an ampere can be obtained without the application of any potential.

In a later form of the experiment a thin rod of carbon about 15 centimeters in length was heated to a very high temperature by low-voltage alternating current; an ionization current of over 3½ amperes was emitted by the surface of the rod to a comparatively cool insulated carbon tube surrounding it, the current-density in this case exceeding 0.1 ampere per square centimeter at atmospheric pressure.

The current appears to be associated with the emission from the hot carbon surface of negatively charged particles. As all the experiments were made at atmospheric pressure, it would seem that these particles must consist almost entirely of sputtered *matter*, and that "corpuscles," whose unencumbered range at such pressures would be extremely small, do not here play the great part in the phenomena they do in high vacua. The evidence to hand in support of this view is discussed in the two memoirs referred to.

In view of the interest of these results obtained with carbon, preliminary experiments on the volatilization and electric emissivity of a number of metals were made, mostly in nitrogen, at pressures from a few millimeters upward. The metals were heated by alternating current, and no applied potential was employed. It was found that positive electricity was emitted at temperatures from 1,000 deg. to 1,400 deg. Cent. With those metals which melt within this range, a sudden and marked increase in the positive current occurred at the liquefying point.

At higher temperatures negative electricity predominates, and its quantity increases rapidly with the temperature. With strips of pure iridium, melting between 2,300 deg. and 2,400 deg. Cent., which was the metal found to give by far the largest emission, the negative current attained nearly 1/10th of an ampere at the melting point, the radiating surface being approximately 4 square centimeters. With an iron transformer plate melting at about 1,500 deg. Cent., a large emissivity was often obtained, but with this metal it was found impossible to repeat any given set of conditions sufficiently accurately to obtain even approximately identical results in successive experiments. The negative currents appear to be always associated with considerable sputtering of the metal, and are probably a consequence of some reaction between the metallic surface and the surrounding gas. With carbon the nature, and to some extent also the pressure, of the gaseous atmosphere surrounding the radiating material exerted an influence on the emissivity at the higher temperatures, and with the metals the temperature at which the positive emission changed sign seemed to be greatly influenced by the presence or absence of minute quantities of impurity in the nitrogen atmosphere surrounding the specimen.

From these experiments, and from the known facts regarding the emissions from other materials such as refractory oxides, it would appear that *most substances capable of withstanding a high temperature emit electricity when strongly heated, and that their emissivity when once established increases rapidly with rise of temperature.*

It was obvious from all the experiments that the electric conductivity inside a hot carbon tube furnace, as evidenced by the ionization currents produced, was very large, and it is of interest to compare the gaseous conductivity thus manifested with other examples.

A hard Röntgen-ray tube has an apparent resistance of megohms. A quartz mercury lamp of the usual type has a resistance when running of the order of 50 ohms. In many of these experiments, even with quite small furnaces, the gaseous resistance was measured to be less than 1 ohm at temperatures above 2,000 deg. Cent.; while in one case in a large furnace, where the temperature was pushed to over 3,000 deg. Cent., the apparent resistance fell to microhms, or at any rate to something comparable with that of the graphite rods and of the copper cables forming the rest of the electric circuit. This would mean that *the conductivity of the ionized space was of the same order as if it were filled with mercury or a liquid electrolyte.*

In none of the emission experiments either on carbon or metals up to their melting point was there any evidence of discontinuity in the law of rapid increase in emissivity with temperature, which was found to hold for the solid substance. Some of the metals, however, showed on melting a marked *increase* in the emission at the moment of fusion.

A word or two may be said here as to the probable velocity and size of the particles which apparently act as carriers in these experiments. In many instances no electric potential was applied, and it seems, therefore, that the particles owe their velocity of emission solely to the temperature. For carbon at 3,000 deg. Cent., according to J. J. Thomson's theory, this velocity should be about 3×10^7 centimeters per second, or about a thousandth of that of light, a speed which, compared with that of some corpuscles, is very slow. But, unlike corpuscles, the particles can travel considerable distances in gases even at atmospheric pressure.

If, as we are inclined to believe, it should ultimately prove to be the case that the ionization currents are carried chiefly by these particles, we have then a means of deducing fairly readily the relation of their charge to their mass (e/m).

An experiment to this end on iridium in nitrogen at 20 millimeters pressure, the strip being at a temperature of 1,360 deg. Cent. (black body), gave the result that the average number of iridium atoms contained in the particle carrying a single negative charge was 1,200. In this case, therefore, the mass of the carrier is roughly 5×10^8 times greater than that of a corpuscle, whose mass is 1–1,800th of that of a hydrogen atom.

Turning now to the phenomena presented in the sun, it would seem in the light of these facts impossible to avoid the conclusion that *the solar atmosphere is a highly conducting medium.* The most modern determinations of solar temperatures all give values between 5,600 deg. and 6,000 deg. Abs.,[4] estimates much lower than those formerly accepted. From spectroscopic evidence it has been established beyond dispute that iron forms one of the principal constituents of the sun, and carbon a not unimportant one both of the sun and of many stars. For present purposes we will limit our considerations to a discussion of the probable influence on the solar atmosphere of the presence of these two elements.

Iron at its melting point gave negative currents up to about 15 micro-amperes per square centimeter in nitrogen at 9 millimeters pressure; a rise of temperature of only 60 deg. Cent. serving to double the current density over this part of the range. Even assuming no further increase of emissivity with rise of temperature beyond the melting point, the current emitted per square kilometer of the area of the photosphere would even on this basis attain 150,000 amperes.

Considering next the case of carbon, if we assume the validity for extrapolation purposes of any such law of increase of conductivity of the space inside a carbon furnace, as that deducible from the data plotted in Fig. 4 of the paper alluded to (Harker and Kaye, *loc. cit.; Proc. Roy. Soc.*, 1912, lxxxvi., A, 385), we find that at a temperature far short of that of the sun the conductivity becomes greater than that of any known substance.

Suppose, however, that, to be on the safe side, we assume in the sun no further increase in emissivity above the figure of 0.1 ampere per square centimeter, actually measured at a temperature approximately 3,000 deg. Cent., this would be more than ample to account for the enormous currents which recent considerations have rendered it probable are circulating in the solar atmosphere.

Hale has shown that it is highly probable that intense magnetic fields of from 2,000 to 5,000 C.G.S. units are found in sun spots, and that there is also a general magnetic field of much less intensity existing over the whole body of the sun. Assuming that the circuit producing the field in a spot consists of a single turn, calculation shows that currents of the order of ten million million amperes are required.[5]

But at the rate of 0.1 ampere per square centimeter, which is 1,000 million amperes per square kilometer, an area of only 100×100 kilometers would alone be required to generate this current, assuming carbon as its source.

As to the mechanism by which the currents *are actually generated* in the solar atmosphere, our present knowledge is very meager, particularly in view of the great uncertainties as to the physical state of the elements in the sun at the prevailing pressures, also as regards temperature distribution over the surface of the photosphere, and radially outward through the solar atmosphere. Of the several alternative processes by which the enormous local currents circulating round a sun spot *might* be generated, that favored by Hale, the revolution of large numbers of the charged particles by the solar vortices, seems least open to objection.

One obvious direction for further research before the data here discussed can be properly applied to the elaboration of an electrical solar theory, is the continuation of the experiments on metals beyond their melting and boiling points. Indeed, a few preliminary experiments in this direction have already been made. One obstacle at present in the way of their successful com-

*From *Monthly Notices of the Royal Astronomical Society*, lxxxiii, No. 8.

[1]Harker and Kaye, "On the Emission of Electricity from Carbon at High Temperatures," *Proc. Roy Soc.*, 1912, lxxxvi. [A], 379; "On the Electric Emissivity and Disintegration of Hot Metals," *Proc. Roy. Soc.*, June 19th, 1913.

[2]Positive emissions were obtained with new carbon at low temperatures on first heating, and were probably associated with the expulsion of the first traces of the contained impurities.

[3]The hot fixed electrode, being in thermal equilibrium with its surroundings, takes no part in the generation of these currents. The latter may be considered as entirely due to the alternating temperature differences between the surface of the movable electrode and the hotter and colder parts of the furnace-wall immediately surrounding it. It has been conclusively established that the ionization currents are *not* due to thermo-electricity, or to any rectifying action of the carbon on the alternating currents used for heating the furnace. Many of the effects have been obtained in experiments where the heat-supply has been furnished from non-electrical sources.

[4]For example, Abbot, "Measurements on Mount Wilson and Mount Whitney," 5,840 deg. Abs. Calculated from an assumed value of Stefan's constant. Also Harker and Blackie, "National Physical Laboratory Report," 1908. 5,610 deg. Abs. By total radiation pyrometer directly standardized up to arc temperature, assuming atmospheric absorption 29 per cent.

[5]The Astronomer Royal and Mr. Maunder kindly supplied the necessary dimensional data for the spots to enable this computation to be made.

pletion is to find a material suitable for the construction of the necessary vessels, which is at the same time sufficiently inert and refractory, and which does not itself emit electricity over the temperature range to be studied.

Some indirect evidence on this question of the behavior of vapors is, however, to hand. It was found that in the experiments with carbon, when a new carbon tube of ordinary commercial quality was heated for the first time, the emission at temperatures in the neighborhood of 2,000 deg. Cent. was always four or five times as great as in later heatings to the same temperature. It was always observed that, during the first heating in a current of inert gas, the impurities in the carbon, consisting mostly of iron and silicon, are distilled out of the body of the material and carried off in the stream of gas. It was ascertained that this comparatively volatile material all leaves the carbon surface carrying a charge.

In bringing the foregoing facts to the notice of astronomers and cosmical physicists, the writer is fully conscious of many criticisms which may be urged against the conceptions here outlined. But in view of the discoveries of Hale and others, and the consequent interest now being taken in solar phenomena, a presentation of the data even at this early stage may not be considered premature.

It may ultimately prove to be the case that such evidence as is here presented, if further supported, may be considered by the authorities as sufficiently conclusive for presuming the existence in the solar atmosphere in very large numbers of electrified particles, by the movements of which enormous currents are generated. Very probably these particles have a mass and velocity widely different from those of corpuscles, the bodies hitherto considered almost exclusively in discussions as to solar currents and magnetism.* It may, therefore, as a consequence, be necessary for cosmical physicists to revise some of their arguments as to the inter-relations of terrestrial and solar magnetic phenomena.

In conclusion, I desire to express my thanks to the Astronomer Royal, to Professor Fowler, and to Dr. W. J. S. Lockyer for their help in references to data and literature.

*See, for example, Schuster, *Porc. Roy. Soc.*, 1911, [1], p. 44; and Birkeland, "The Norwegian Aurora Polaris Expedition," vol. I.; "On the Cause of Magnetic Storms and the Origin of Terrestrial Magnetism," p. 151 *et seq.*

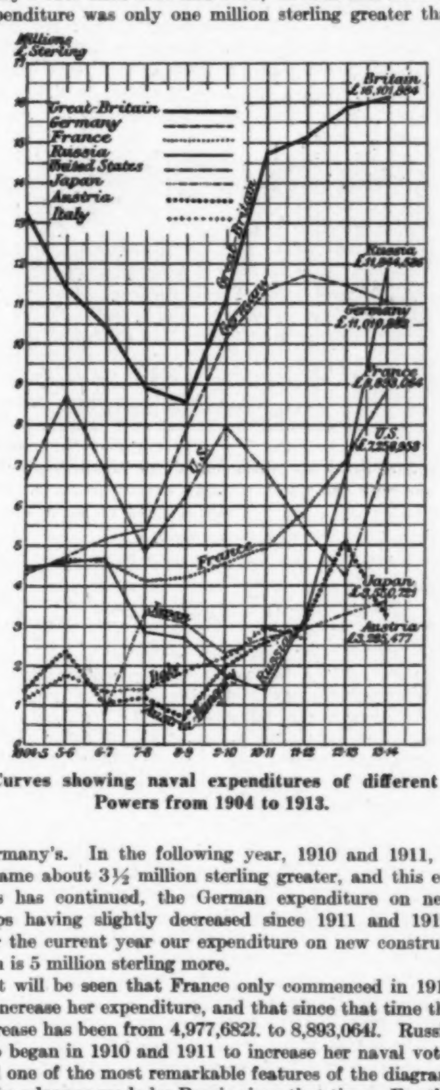
Naval Expenditure of the Powers*

The British Admiralty Get Better Value for Cost of Construction Than Do Other Powers

The contention of the First Lord of the Admiralty, in his recent speech at Manchester, that the expenditure on our Navy now bore a less proportion to our oversea trade and national revenue than in the past years was of greater significance, and, perhaps, will be of more lasting influence, than his suggestion that we should not lay down any of the four battleships of next year's program if Germany agreed not to begin the construction of the two embraced in the program according to their Navy Law. The latter idea may be dismissed in the words of a semi-official communication issued from Berlin:—"Mr. Churchill has found little support either in the English or in the German press. Whatever may be adduced against the practicability of Mr. Churchill's idea, there can be no doubt about the English Naval Minister's good intention and the honesty of his utterances. Mr. Churchill did not say a word that could damage the friendly development of Anglo-German relations." There are many reasons why Germany should not accept the proposal. In the first place, Germany is not our only possible combatant, nor are we the only possible combatant of Germany. The foreign ships building in England would afford us an advantage in the event of trouble arising, because these could then be commandeered for His Majesty's Service. Moreover, battleships, to which Mr. Churchill's proposals are confined, constitute only a part of the fighting fleet. These conditions alone create grave difficulty in making an arrangement for cessation of battleship keel-laying even for one year. Admiral von Tirpitz, the chief of the German Admiralty, has this week given his dictum that the German Navy Law will be carried on to its conclusion without hesitation.

The annual return showing the total naval expenditure during the past ten years, issued this week, throws considerable light on the subject of proposals for temporary cessation of battleship keel-laying. The most suggestive figures in the return are those applicable to the expenditure on ships, propelling machinery, armor, and armament; in other words, the votes of the different Powers for the building and equipping of all types of warships. By taking these alone, it is possible to eliminate variants and get down to the bedrock of naval strength. It is true that constructional work is undoubtedly less expensive in this country than in any other; consequently our expenditure represents a greater degree of fighting strength. It is not advisable to make deductions on this score on the cost per ton of displacement of a ship when completed, because this displacement includes fuel and stores, and the result is vitiated by variation in practice as to the weight of fuel and stores carried by each ship. The tonnage of a ship ready for fighting, without such variable quantities as fuel and stores, is not available, and thus we are forced to be satisfied with the general conclusion that the British Admiralty get from 10 to 20 per cent greater value for expenditure on new construction than do other Powers. Instead of giving the figures for each nation, we have plotted them on the diagram which accompanies this article.

It will be seen that in the first three years of the decade embraced in the Government return, practically all Powers decreased their expenditure on munitions of naval warfare, except only Japan, which embarked on a program of new construction in order to strenthen its fleet after the war with Russia. Of the Continental Powers, Germany was the first to show a marked increase in expenditure on warships. Their total for the year 1908 and 1909 was nearly 1,900,000*l.* greater than in the previous year, and in 1909 and 1910 there was a further increase, the expenditure in two years having gone up from 5,910,959*l.* to 10,177,062*l.* The expenditure for the United States, it will be seen, has been a markedly fluctuating one, and may be left out of the reckoning. No immediate response to the German action is visible in the expenditure curve for France, Japan, and Russia. But this country recognized in the action of Germany a distinct reason for increasing expenditure on new construction. When one measures the relative need for naval construction in Germany and Britain in the light of the comparative extent of sea coast, size of merchant fleet and volume of over-sea trade of the two countries, the fact that Germany's expenditure on new warships was within 865,000*l.* of our expenditure of 1908 and 1909 justifies the sharp advance in the British expenditure. From this time forward there was a rapid rise in Germany's vote until 1910 and 1911; in 1909 and 1910 our expenditure was only one million sterling greater than



Curves showing naval expenditures of different Powers from 1904 to 1913.

Germany's. In the following year, 1910 and 1911, it became about 3½ million sterling greater, and this excess has continued, the German expenditure on new ships having slightly decreased since 1911 and 1912. For the current year our expenditure on new construction is 5 million sterling more.

It will be seen that France only commenced in 1910 to increase her expenditure, and that since that time the increase has been from 4,977,682*l.* to 8,893,064*l.* Russia also began in 1910 and 1911 to increase her naval vote, and one of the most remarkable features of the diagram is the advance made by Russia since that time. For the current year her expenditure is next to that of Britain, exceeding Germany's by 834,000*l.* Indeed, the available building resources of Russia are overstrained, and it is said that the Admiralty is unable to spend the vote granted it by the Duma. The extraordinary increase in the Russian expenditure is due to the fact that a great leeway has had to be made up for well-known reasons, and Russia has, rightly or otherwise, decided that all the ships should be built within the country. There is suggestion that a still further increase should be made by the beginning of a ten years' program, involving an expenditure of 100,000,000*l.* for the construction of eight dreadnoughts and the other units required to make up a fighting squadron. In this fact there is probably sufficient reason for hesitancy on the part of Germany to reduce the Navy program already agreed upon by the Reichstag. Italy and Austria-Hungary are undoubtedly important factors in the situation, but figures for Italy's new construction for the past two years are not available, according to the British Government return; so far as they are given they are included in the diagram. The Italian expenditure has greatly increased probably at as great, if not a greater, rate than the Austro-Hungarian expenditure. For new construction the expenditure of the latter has doubled since 1910, being, for 1913, 3,285,477*l.*, while in 1912 it was 5,105,000*l.*, and in 1911 3,125,000*l.* The average for the three years was thus 3.8 millions sterling, whereas in 1906 and 1908 it was under 1 million sterling. We are thus fast moving towards a competition in naval expenditure in the Mediterranean as well as in the North Sea.

*Engineering.

The Modern Blast Furnace Burden

According to the official statistics the manufacture of 29,726,937 gross tons of pig iron in 1912 involved the consumption of about 55,656,000 gross tons of domestic and foreign iron ore, ore briquets, etc., and about 4,319,000 tons of mill cinder, scale, scrap, slag, zinc residuum, etc. As the latter item is only about one thirteenth the former it does no great violence to add the two together, and compute that the average furnace burden was equivalent to 2.017 tons of ore per ton of pig iron manufactured, making a yield of a shade under 50 per cent. Without undertaking to go into details as to the loss of metallic iron in slag, flue dust, etc., it is evident that the average iron content of the ores used was less than 50 per cent, since the average pig iron contains only about 95 per cent iron.

Between 80 and 85 per cent of all the pig iron of the United States is made from Lake Superior ores, while the imported ores are of relatively high grade, leaving it that the comparatively low-grade ores used by southern furnaces do not operate greatly to pull down the average yield. The showing is much less favorable than would have been made, say, fifteen years ago, when the cream of the Lake Superior deposits was being used. Unfortunately there are no statistics for such years, but it is readily recalled that ores running well above 60 per cent were the rule rather than the exception.

The ores have been changing and the conditions of operation change with them. The pig iron producer adapts himself to the changes without inconvenience. The difference in conditions appears on the surface not in the form of its having become more difficult to operate a blast furnace, but from its being impossible to produce pig iron as cheaply as formerly. More ore must be mined and transported, and more fuel and flux must be consumed, simply adding to the expense.

It should be noted that the gradual exhaustion of the Lake Superior deposits which were first selected for exploitation does not always lead to the mining of a lower grade ore. There are three factors, the iron content, the character of the impurities, and the cost of mining. In the early days of the Mesabi range, when ore was literally "as cheap as dirt"—certainly if one undertook to place orders for "dirt" in hundred-thousand-ton units he would find it rather expensive—if a 60 per cent ore lay in 100-foot thickness on the surface and a 65 per cent ore of the same thickness lay under 100 feet of overburden the former would have been mined and the latter left as impractical at the time, but to-day conditions would be different. Very good ores are now being mined, at great outlay of capital in removing great overburdens, and thus the exhaustion of the earlier mined deposits leads in one direction to the mining of lower-grade ores, but in another direction to the mining of deposits which are very good, but expensive to get at.

The ease with which pig iron is made with a much more expensive burden than that of fifteen years ago suggests how easy it will be to continue using lower and lower grade ores. We shall make tens of millions of tons of pig iron, perhaps nearly a hundred million tons, for every unit that the average ore used loses in iron content, and in making that pig iron we shall obtain ample experience to attack the new problems successfully. Inasmuch as the visible supply of iron ore increases very rapidly for each notch that the limit of commercial availability is dropped, it is clear that iron for future generations can readily be found. A higher price, measured by unit of endeavor, will have to be paid, but through the endeavor being better directed the cost will not be enhanced in proportion. If the cost should advance when measured by dollars per ton, the public will have increased ability to pay.—*Metallurgical and Chemical Engineering.*

The Pearling Industry*

A Chapter in Economic Biology

By H. Lyster Jameson, M.A., D.Sc., Ph.D.

In this paper I propose to review, very briefly, the more important attempts that have been made in recent years to apply the science of Marine Zoölogy to the solution of the economic problems presented by the pearl and mother-of-pearl fishing industries, in different parts of the world. The present excessively high price of pearls, and the frequent substitution for them of imitation articles, even among classes who would scorn to wear paste imitations of mineral gems, and still more the amazing price to which the best qualities of mother-of-pearl shell have risen (the best lots reached £550 per ton at the recent London sales) all emphasize the fact that, so far, we zoölogists have not been able to devise a method for increasing production, or for restoring depleted beds of pearl and mother-of-pearl oysters. And yet it cannot be said that we have been stinted for support, or that governments and financiers have in all cases turned a deaf ear to our proposals. Accordingly, while reviewing the work that has been done, I propose to set forth a few ideas, formed as a result of a study of these problems extending over some thirteen years, as to the causes of the small response Biology has made to the demands of industry in this case.

The chief localities in which biologists and business men have concerned themselves with the question of the application of biological knowledge and theory to this industry are Japan, Mexico, the French possessions in the Eastern Pacific, Burma, the Red Sea, Ceylon, and Australia.

JAPAN.

Japan has gone ahead of all the Western nations and their colonies and possessions in being the first country to establish a pearl-farming industry, based upon a scientific knowledge of the biology and physiology of the mollusc, which has proved itself, after years of trial, to be a firmly founded and highly remunerative business. The two names which are particularly identified with the development of this industry are those of the late Prof. Mitsukuri and Mr. K. Mikimoto, an ideal association of the learned scholar and the far-seeing business man. These two pioneers met first at the National Industrial Exhibition in Tokyo in 1890, where Mr. Mikimoto, a pearl merchant, had an exhibit of pearl oysters (*Margaritifera martensii*) from Japanese waters. It was then that Prof. Mitsukuri suggested to Mr. Mikimoto the possibility of cultivating the pearl oysters and making them produce pearls.

When Mr. Mikimoto started practical work on the Shima fisheries, he shared the common fate of prophets and pioneers, and was ridiculed by his friends for "throwing his money into the sea." However, meeting and overcoming, one after another, the difficulties that are inseparable from the early stages of such an enterprise, turning for advice to Prof. Mitsukuri and Dr. Kishinoue, retaining unshaken his faith in the ultimate attainment of his goal, he saw, within six years of his first meeting with the Professor, his enterprise pass from the experimental to the commercial scale, patented his process, and, at the end of 1898, marketed his first crop of "Culture-Pearls," as these products were named.

The enterprise is carried on in the Bay of Agu, in the province of Shima, and the area leased for that purpose, which amounted to about five hundred acres in 1904, one thousand acres in 1905, is stated to have been extended in 1911 to about twenty-two nautical miles. In 1911 it supported fifty families, whose headquarters is a village situated on a previously uninhabited island.

The operations consist in collecting the young oysters on stones, which are laid down, just before the ascertained spatting season, in areas where there is an abundant spatfall; in laying out the young oysters so collected on more suitable grounds, and, when they have attained a certain size, in operating on them to induce them to produce pearl-like excrescences or blisters. This is done by introducing between the body of the oyster and the shell a bead of mother-of-pearl, which, in the course of time (four years in Japan) becomes coated over with nacre, giving a hemispherical or more than hemispherical pearl-like body, or "Culture-Pearl."

In 1905 the number of oysters operated on per year was from two hundred and fifty thousand to three hundred thousand; but I fancy it is very much larger now. These blisters are used in cheap jewellery for purposes to which half pearls are applicable. I have not figures as to the present production, but it must be very large, as they are becoming extremely common in rings, scarf-pins, studs, and so on in Europe. They are not *pearls*, but *blisters*, and as such their value is small compared with that of real pearls of comparable size. Indeed, their low value leads me to think that it would scarcely be worth attempting to produce them in the majority of British pearl-shell producing colonies, where the labor and other conditions are difficult compared with Japan. There is every reason to think that Saville-Kent's enterprise with *Margaritifera maxima* in Torres Straits, though the production of "pearls" was spoken of, was in reality concerned with producing these "blisters." It has been shown over and over again that *Margaritifera maxima* and other species can be successfully treated after the Japanese method, and it may be added that in the case of *M. maxima* and *M. margaritifera* the growth of the shell is so rapid that the blisters can be produced in a fraction of the time that is required in the case of the Japanese oyster. I am informed that blisters produced in this way in *M. maxima* are being marketed now, in Paris and the United States. Although it has been stated that fancy prices have been given for a few of these, I believe their value will fall to a level comparable to the price of the Japanese article, as soon as their nature is understood by the public; and their production will, in consequence, be found to be unprofitable in the majority of cases.

Fig. 1.—Section of the shell of the Japanese pearl oyster showing a "culture pearl" attached.

It is, however, worth while considering whether it would not be practicable (provided conservation was possible) to establish a "Culture-Pearl" industry in some of the rivers frequented by the freshwater pearl mussel in these islands; if, as is possible, there are still areas where all desire for rural home industries has not yet disappeared. The practicability of producing fine blisters in our freshwater pearl mussel was proved by Linnaeus, whose specimens can still be seen at the Linnean Society, including some that are in no way inferior to the Japanese ones. I do not wish to convey the impression that we have here the potentiality of a highly lucrative industry, in which fortunes can be made, but I think the widely-spreading habit of wearing imitation jewellery affords an ample guarantee of a steady market for a product which occupies a position intermediate between the real pearl and the glass and paste imitation, and which, consequently, will meet the needs of those who cannot afford to buy the former, and whose self-respect forbids them to wear the latter.

Most of the people who have produced blisters in this way have hoped to obtain real spherical "pearls" by the same method, or a modification of it. Quite recently Mr. Toyozo Kobayashi, Professor at the Tokyo Higher Technical College, who is associated with Mr. Mikimoto in his enterprise, has informed me that Mr. Mikimoto has produced a few perfectly free spherical pearls in this way, but that the process is too uncertain to be applicable, as yet, on a commercial scale.

I have always held that a modification of the Japanese process could be devised that would yield this result. But I maintain, in view of what is known of the nature of real pearls, that such bodies would not be "pearls" in the strict sense, and I am of opinion that they could be distinguished from the real article. In fact, I doubt very much whether they could legally be marketed as "pearls."

MEXICO.

For many years past work has been carried on with a view to the cultivation of the pearl oyster of the Pacific Coast of America, *Margaritifera margaritifera* var. *mazatlanica*, in the Gulf of California; but unfortunately no satisfactory scientific account of these operations is in existence. The chief company concerned with this enterprise is the "Compania Criadora de Concha y Perla de la Baja California" and the work was initiated by Mr. Gaston Vives, who has been studying the subject for many years. The chief work of the company is transplantation, in which considerable success is claimed, and it appears that elaborate devices are used to protect the young oysters during the early attached stages, on lines not unlike those adopted for the same purpose with the edible oyster in Holland. Efforts have also, apparently, been made to propagate the oysters; but I am not aware that this has proved feasible on a commercial scale.

So far as I know, apart from transplantation, the work has not yet reached the stage which would warrant its being called a commercial success. In 1909 no less than one hundred and fifty thousand dollars had been spent on the enterprise.

BURMA.

In 1907, when there was a boom in scientific work, owing to the successful promotion and large initial dividend of the Ceylon Company of Pearl Fishers, Prof. Herdman was approached by the Burma Government with a view to a biological inquiry there. Prof. Herdman assigned the work to two young biologists,

* A paper read before Section D (Zoölogy) of the British Association for the Advancement of Science at Dundee, and published in *Knowledge*.

Fig. 2.—"Lingah" shell (M. vulgaris Schumacher), from Torres Straits.

Fig. 3.—M. panasesae Jameson. The "False Spat" that occurs associated with the black-lipped shell.

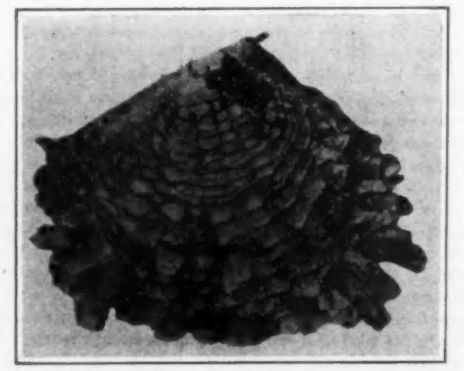


Fig. 4.—Young of the black-lipped mother-of-pearl oyster (M. margaritifera Linn). Conflict Atoll, Papua.

Fig. 5.—Bastard shell (M. sugillata, Reeve) from Torres Straits.

who proceeded to Burma and published a report, which, considering the limited time at their disposal, is an excellent survey of the situation.

No very definite proposals were made for applying biological science with a view to increasing production; but the question of the repopulation of the banks was discussed on pages 18 and 19, and it was suggested that breeding stock should be laid down in a sheltered bay, and the young collected. The objection to this proposal is the impossibility, in almost every case, of securing that the spat, which passes through a fairly long plankton stage in waters where there is often a strong tidal current, will return to the neighborhood of the parent shells to attach itself.

I must here mention a matter which is regrettable. It is an instance where a regulation has apparently been based upon a scientific theory which is probably erroneous, and consequently the regulation instead of being beneficial is, if anything, harmful. In the "Rules for Lower Burma, under the Burma Fisheries Act, 1905," Sections 64 and 67, the taking of the oyster-eating fishes, *Balistes* and *Trygon* in the pearl fishery districts was prohibited, and fishermen were required, if they caught these fishes accidentally, to return them to the sea. These rules were issued in August, 1907, just after the above-mentioned report was published. They have since been repealed. It is only fair to the authors to say that the recommendation that such a regulation should be framed is not contained in their report; indeed, it is difficult to imagine how and by whom such a recommendation could have been made. At that time these fishes were supposed to harbor the intermediate and adult stages of a worm which was supposed to cause the formation of pearls in the Ceylon pearl oyster, *Margaritifera vulgaris*, but I do not think there was any evidence that the same parasite occurred in the widely different Mergui mother-of-pearl shell, *Margaritifera maxima*. And my subsequent researches have shown that, apart from this rash analogy, the idea that this worm causes pearls even in the Ceylon oyster is highly doubtful. The regulation thus had the effect of protecting what are probably two of the worst enemies of the oyster.

Mr. John I. Solomon, who is not a trained biologist but an engineer, has formed the Burma Shell Company, and started work in the Mergui Archipelago. An attempt was made by him to grow mother-of-pearl oysters in a large tank on the shore of an island; but this was, as might have been expected, unsuccessful. Some success has, however, been achieved in producing blisters on lines similar to those followed in Japan. I have seen some of Mr. Solomon's products, and they are the finest artificially-produced blisters that I have yet met with, and I understand that he is now marketing them; but whether the enterprise will prove commercially successful will depend, in the main, on whether it will be feasible in those waters to produce these commodities at a profit when they fall to a value comparable to that of the Japanese article, as they must do as soon as their nature is understood.

RED SEA.

For some years Mr. Cyril Crossland has been experimenting in the Red Sea for the Sudan Government, studying the marine biology of its waters, with special reference to the three species of Margaritifera that occur therein, viz, *M. margaritifera* var. *erythreae*, *M. vulgaris*, and the valueless *M. mauritii*. So far as I know, Mr. Crossland has not yet published an account of his economic work.

FRENCH PACIFIC.

The question of cultivating the "Tahiti" mother-of-pearl oyster (*M. margaritifera* var. *cumingii*) has often been broached, and has been the subject of several scientific missions; but without any considerable results.

Space forbids me to deal with the several missions in detail in this paper.

CEYLON.

My account of the scientific work done here in the last dozen years will be very brief, as I have already dealt with it recently in two papers (*Journal of Economic Biology*, February, 1912, pages 10 to 22, and *Proceedings of the Zoological Society*, 1912, pages 260 to 358, plates XXXIII, XLVII).

The history of the enterprise is briefly as follows: In 1900 the Ceylon Government, anxious to devise measures for preventing the frequent occurrence of barren years or periods of years, approached the Council of the Royal Society and Prof. (now Sir) Ray Lankester, with a view to obtaining scientific advice. As a result, Prof. Herdman was sent on a mission to Ceylon, and left behind him, after a couple of month's work on the spot, an assistant to carry on the work initiated by him. Later on the work started by the Government was taken over by the Ceylon Company of Pearl Fishers, Ltd., a company formed largely to give effect to the recommendations made as a result of this mission. The capital of the company was $825,000, and Sir West Ridgeway, who was Governor of Ceylon when the mission was undertaken, became chairman of the company. Prof. Herdman was retained as scientific adviser to the company. Briefly summarized the

Fig. 7.—Plan of Tidal Pond, showing the course of the current.

position may be stated as follows: The two remedies recommended as a result of the scientific mission, viz., cultching and transplantation, have so far failed in practice, and the company is now in liquidation. Moreover, as I have shown elsewhere, the most important scientific discovery claimed, that is to say, the Cestode

Fig. 6.—Young of the Australian mother-of-pearl oyster (M. maxima Jameson). Montebello Island.

origin of pearls, is probably a mistake. Extensive faunistic data were collected; but the relation of some of these to the main question is far from obvious.

A speech made by Sir West Ridgeway, on October 27th, 1900, after a paper by the late Mr. Oliver Collett, on Pearl Oysters, read before the Colombo Branch of the Royal Asiatic Society and reported in *The Ceylon Observer* for October 29th, suggested that Sir Ray Lankester saw, in Ceylon's need for scientific guidance, an opportunity for "enriching the scientific world at the cost of Ceylon;" a charge which is all the more unfortunate because probably a more intensive study of the pearl oyster itself, and of pearl production, would have yielded more immediate practical benefit than the very extensive survey of the marine fauna of Ceylonese waters which forms so large a part of the work which has been done.

A remarkable point in connection with this scientific mission was the fact that (except that I myself was invited by Prof. Herdman to accompany him as assistant), no serious attempt was made, so far as I know, to secure the co-operation, even in a purely critical and advisory capacity, of the several naturalists (foremost among whom was the late Mr. Saville-Kent) who had already studied the problems connected with pearl oysters and pearls. It is, indeed, implied in a letter from Mr. Saville-Kent published in *The Ceylon Observer* of December 28th, 1900, and in a leading article of the same date, that he was deliberately left out of the councils.

I think that almost every incident in connection with the Ceylon affair, which might be used for the purpose of discrediting zoölogical work, could have been avoided if the assistance of specialists had been invoked.

Turning now to the attitude of the scientific press, I think it is to be regretted that an attempt was made to claim, as due to biological work, certain results that were the result of quite other causes. In *Nature* for July 18th, 1907, there is a review of Prof. Herdman's report[1] in which it is implied that the phenomenal success of the four fisheries which followed upon his visit was due to his scientific investigations. On page 271 there is the following statement:

[1] "Report to the government of Ceylon on the Pearl Oyster Fisheries of Manaar." London: Published by Royal Society, 1903-6.

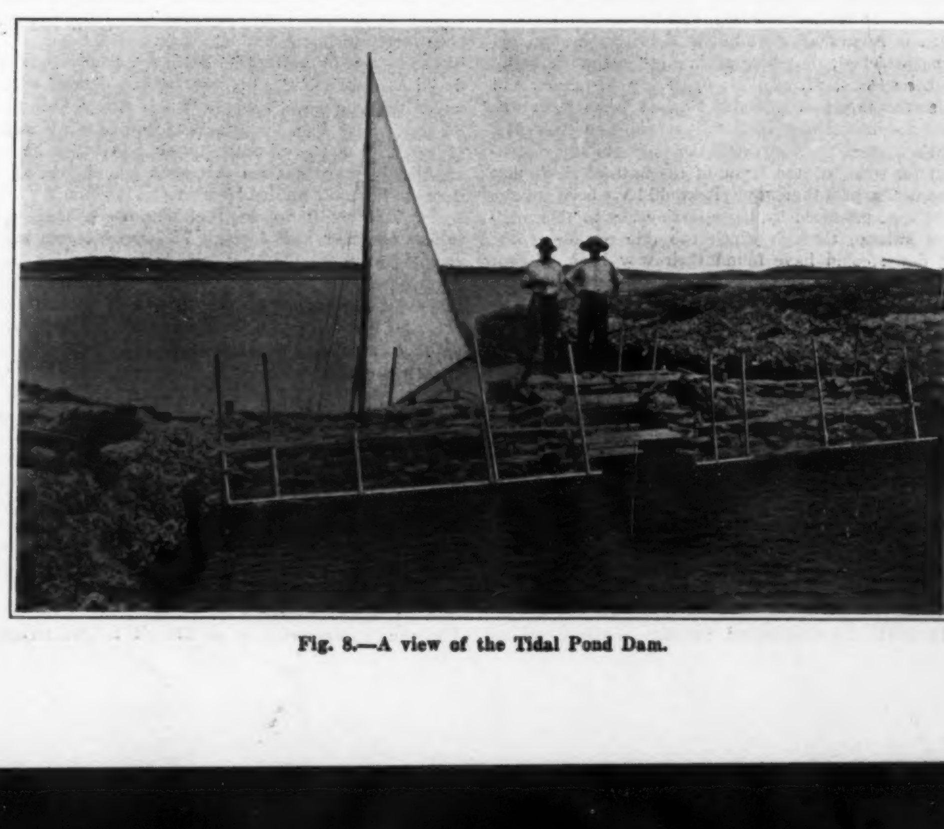


Fig. 8.—A view of the Tidal Pond Dam.

"It is very interesting to find that since Prof. Herdman's expedition there have been four successive fat years of pearl fishing, the most profitable, so far as is known, that have ever been. . . . . This should surely convince the Philistines that there is something in biology after all!"

And on page 272, with reference to some observations of Prof. Herdman's as to the interrelation of biological phenomena:

"If this wise saying were as widely accepted as it is certainly true, biological science would find more generous public support, and we should hear no more of impatient criticisms of scientific investigations which do not yield an increase of rupees so rapidly as Prof. Herdman's study of the Ceylonese Oyster Beds."

I am sure that Prof. Herdman would be the first to disclaim this; these fisheries, like previous ones, were the result of natural deposits of oysters, which matured, were discovered in the course of inspection, and were fished when ripe. It is particularly regrettable, in view of the heavy losses which have been incurred by the investing public through the failure of a company which hoped to achieve great results through the aid of science, that an erroneous idea of this kind should have been circulated by a paper of the standing of *Nature*. The other extreme has been reached by the general public, as instanced in the newspaper reports of the meetings of the company, where the failure of the operations is attributed to the bankruptcy of science, a charge as untrue as it is unjust.

(1) AUSTRALIA.—MR. SAVILLE-KENT'S EARLY WORK.

The first naturalist to make a serious study of the Australian mother-of-pearl oyster (*M. maxima*), the most valuable of all kinds, was the late Mr. Saville-Kent. Mr. Kent demonstrated the feasibility of transplanting this species from the fishing grounds, and of successfully laying it down in shallow inshore waters.

His results in actual cultivation are vitiated by the fact that he mistook the "bastard shells" (*M. vulgaris* and *M. sugillata*) (see Figs. 2 and 5) which can be collected in enormous quantities on suitable catchment in tropical Australia, for the young of *M. maxima* (see Fig. 6). This mistake has been made by almost all investigators of this species. The young shells figured by him in his works, "The Great Barrier Reef" and "The Naturalist in Australia," are certainly "bastard shells" and not the young of *M. maxima*.

Mr. Saville-Kent urged the establishment of cultivation on the foreshore, undersized shell being brought in from the grounds and laid down to grow and reproduce. He considered such a stock would breed and multiply; but I have always held that the man who lays down breeding stock, while no doubt a public benefactor, reaps a very small fragment of the harvest himself, the free-swimming young being scattered far and wide before they settle.

In 1894, when the pioneer shellers who worked the industry from "stations" or homesteads scattered about on the various islands were giving place to highly organized fleets, owned by companies in Sydney and Brisbane, some of the shellers themselves tried to induce the Queensland government to make the bringing in and laying down of small shell compulsory, with a view to founding a permanent white men's industry; but without results.

Mr. Saville-Kent made some experiments in Western Australia in transplanting shell, and he demonstrated that this species could be kept alive so far away from its natural haunts as Shark's Bay. He claims to have bred young oysters from stock he laid down in a mangrove swamp in Roebuck Bay, and figures in his "Naturalist in Australia" the adult shell, with the supposed young attached; but here again the "young" is really another species, *M. sugillata* or *M. carcharianum*, and the assumption that reproduction had taken place was therefore unfounded. In fact, it would seem as if Mr. Kent's oysters had provided the only suitable catchment for some of the larvæ of the bastard shell, that happened to pass that way. It would have been strange if the spat produced by these few oysters in this mangrove swamp, through which the tide regularly ebbs and flows, could have found their way back to their parents, after drifting about for days, or perhaps weeks, at the mercy of the tide. I am surprised that biologists and practical shellers have so often based their hopes on this assumption. The same argument applies to Mr. Saville-Kent's claim that he got this species to breed at Shark's Bay.

As a result of his success in acclimatizing the shell at Shark's Bay, Mr. Kent proposed to introduce it into Houtman's Abrolhos; but I am not aware that this was ever tried. Why people should want to try to introduce this species into extra-tropical localities, for purposes of cultivation, when there are thousands of square miles of suitable ground for this enterprise in the natural haunts of the oyster, I cannot conceive.

(2) THE PILOT CULTIVATION COMPANY'S ENTERPRISE IN TORRES STRAITS.

In 1891, the Queensland legislature passed an act permitting the taking of undersized mother-of-pearl oysters; that is to say, examples of less than five inches, nacre measurement, for cultivation purposes (this privilege has since been cancelled). The Pilot Cultivation Company was formed by some of the leading pearling-fleet owners, Mr. James Clark being the moving spirit. An area about two miles long, and varying in width from half a mile to a mile, in the passage between Prince of Wales Island and Friday Island, was leased to the company. This passage was well known to Mr. Clark, who had formerly had a station there; so that, so far as the experience of a practical man is a guarantee, it may be assumed that it was a suitable place for this enterprise. It had formerly carried rich deposits of shell, which, however, had been cleared off.

The following account of the enterprise is in great part pieced together from what was told me when I visited Torres Straits in 1900, and from the evidence given before the several commissions on the pearling industry. It is difficult to get accurate information, or exact data; indeed, it seems probable that there was some intentional reticence on the part of those witnesses who were personally interested in the venture.

The first attempts at transplanting oysters were made with a sailing vessel, and were not successful, owing to heavy losses, attributed to insufficient circulation of water in the well in which the shells were carried; but with the substitution of steam for sailing power this difficulty was overcome. The oysters were carried on trays in the hold, and the water was kept in circulation by a powerful pump. The loss in transit was estimated to be not more than two and a half per cent, except where delays occurred or the shells were overcrowded on the trays.

I have been unable to obtain exact dates and figures; but it appears that the laying down of shell began in earnest in May, 1894, and from that date something between one hundred thousand and one hundred and fifty thousand young shells, from the area known as the Old Grounds, collected by the divers in the ordinary course of their fishing operations, were laid down. Most of them were under five inches in nacre measurement. It appears from the evidence of a diver employed in the operations that the transplanted shells were simply dumped overboard on the leased area, and no care was taken to lay them down in suitable positions. This procedure cannot be recommended to future cultivators. The shells so laid down were inspected periodically. A lot of dead ones were found, especially among those that had happened to fall on unsuitable ground. The numbers that died ran into many thousands, and it appears that twenty-four cases (about three tons) of dead shell were collected in the course of the experiment.

In August and September, 1897, seven weeks were devoted to fishing these oysters, and thirty thousand or thirty-five thousand were taken up; those marketed exceeded six and a half inches nacre measurement (one thousand four hundred to the ton). As in other cultivation experiments, high hopes were raised by the appearance of quantities of what seemed to be young mother-of-pearl oysters, on the leased area; but these proved to be bastard shell.

A few oysters were left on the ground. I have in my collection one that was taken up in 1900 when I was in Torres Straits.

The law afforded the company insufficient protection, it being found that it was impossible to convict poachers; the court holding that pearl oysters were *feras naturae*, a defect which has, I believe, since been remedied in the criminal code.

About the beginning of 1897 the company introduced a biologist, Mr. S. Pace, who had been trained under the late Prof. Howes, at the Royal College of Science. Mr. Pace had had no previous experience of pearl or mother-of-pearl oysters. At first he was engaged in investigations at Goode Island, but subsequently he took up his quarters on the hulk "Dayspring," a vessel which, beginning as a missionary barkantine, had had a highly checkered career before she was moored in Friday Island Passage, as a floating biological laboratory. Mr. Pace has never published an account of his work; but it appears from evidence laid before the Commissions by his employers that such results as he achieved were of academic rather than practical value.

In a report published in the Thursday Island Government Resident's Report for 1898, Mr. Pace proposed the construction of a tank or incubator, perhaps by damming the ends of a channel, in which hand-fertilized ova were to be carried through the pelagic stage, and caught on collectors. Paper anticipations of this kind are common in connection with proposals for the cultivation of mother-of-pearl oysters and edible oysters, but the practical success of such devices has yet to be demonstrated.

With regard to the cost of the Pilot Cultivation Company's enterprise, it is difficult to get anything like satisfactory figures. It is generally claimed by those who carried through the work that it meant a heavy loss to them, and this may be so, if futile experiments and scientific work carried on for an insufficient period be debited against the returns. But, it is probable that the actual cost of collecting, transplanting and raising the shell was considerably more than covered by the price realized by sales.

The work done by this company can hardly be called cultivation. It was simply removing young and undersized shell from the natural grounds and laying them down in more sheltered waters, in order that when they had grown to a profitable size or to a size at which they could be marketed without infringing regulations, they might be economically harvested. As to the scientific investigations, it was absurd to imagine that the problems could be solved in a couple of years by a young naturalist, fresh from college and without previous experience. If success could be achieved so easily as this the prospects of this industry would indeed be alluring!

(3) MY EXPERIMENTS IN PAPUA, IN 1899-1900.

From November, 1899, to August, 1900, I was engaged by the lessees of the Conflict Atoll in experimenting with a view to the cultivation of the black-edged mother-of-pearl oyster, *M. margaritifera*, and the introduction into the lagoon of the large mother-of-pearl oyster, *M. maxima*, which occurs around the mainland and larger islands. In the case of the former species, the difficulty centered around the impracticability of obtaining spat in sufficient quantities, the great bulk of the supposed spat obtained on collectors proving to be a worthless species, *M. panasseae*. With regard to *M. maxima*, it does not, in my experience, frequent pure atoll formations, its occurrence in Eastern British New Guinea being confined to the neighborhood of the mainland, and of those islands which are of a formation other than recent coral.

Several consignments of this species were laid down. Attempts to secure spat from them were not successful, and, while my observations did not extend over a sufficient period to warrant a dogmatic statement, all the indications point to the conclusion that proposals to establish this species in atoll formations, where it is not native, are not likely to meet with success, and I urged strongly at the time that, if the cultivation of this species was to be undertaken seriously, a suitable site on its native grounds should be secured.

The apparent unsuitability of atoll lagoons for this species is perhaps associated with the higher salinity of the water, and the absence of river influence; for my studies lead me to believe that, while an excess of fresh water is injurious to this species, it normally frequents localities where the water is, occasionally or regularly, influenced by minute traces of river water and the detritus which it carries with it.

(4) MR. J. R. TOSH'S WORK FOR THE QUEENSLAND GOVERNMENT.

Mr. Tosh, who had been trained under Prof. M'Intosh, at St. Andrew's University, went out in June, 1900, and made Thursday Island his headquarters. There is practically no published record of what he achieved, and there is some reason to think he received inadequate support from the Government. He and I laid down a few young pearl oysters at Badu, in September, 1900, with a view to obtaining growth data; but I do not think that these were ever recovered. In 1901, he made proposals for the erection of a laboratory and "hatchery" at Wai Weer, a small island about two miles distant from Thursday Island; but effect was not given to his proposal, although I believe tenders were called for. The laboratory was to include three concrete tanks for experimental work (see note by Prof. M'Intosh, in *Nature*, August 15th, 1901), and it is probable that these constituted the "hatchery," another of the many barren proposals to raise oysters in tanks. Mr. Tosh published a report, of a purely administrative nature, in the Report of the Queensland Marine Department for 1900 and 1901. The principal recommendations were the closing and opening of the grounds in rotation, as is done in the French Pacific, the gradual reduction of the number of boats licensed, and the raising of the size limit to six inches. He also advocated the adoption of the six-inch limit for the black-lipped shell, which to anyone familiar with that species in Queensland waters will appear quite absurd.

Mr. Millman, the Government Resident at Thursday Island, in his Report for the year 1904, stated that Mr. Tosh "by actual experiment arrived at the knowledge that, given suitable tanks or docks, something like seventy-five per cent of the enormous number of spat might be saved, and would on removal to proper beds arrive at maturity." If Mr. Tosh achieved this result he has done what practically all other experimenters have failed to do with this and other species, despite many and elaborate experiments. Mr. Millman goes on to say that Mr. Tosh's services were "dispensed with at a time when he was about to fully demonstrate the method of securing and saving

the spat emitted in such enormous numbers." It appears in Mr. Millman's report that it was proposed to raise spat in the hatchery at Wai Weer, and to sell the young shell, at six months old, to cultivators, to be laid down on their concessions till it should reach a marketable size. The artificial production of "pearls," apparently on the Japanese lines, was also a part of the scheme, as outlined by Mr. Millman. Mr. Millman also made the statement (I do not know on what authority) that the oyster changes its sex every year; this I believe to be unfounded.

(5) MR. SAVILLE-KENT'S ENTERPRISE IN 1908.

About the year 1906 Mr. Saville-Kent obtained a concession for cultivation purposes near Somerset, in Torres Straits, and shortly afterward formed the Natural Pearl Shell Cultivation Company, Limited, to carry on the cultivation of mother-of-pearl oysters, and the forced production of pearls, according to a "secret process" which he claimed to possess. Mr. Kent treated some oysters for pearl production in December, 1907, and the more extensive work appears to have commenced in February, 1908. Spat collectors were laid down, and quantities of spat were collected, which were, in all probability, only the valueless "bastard" shell. Between May and July of the same year about two thousand oysters were laid down, apparently as breeding stock, with a view to collecting the spat produced by them, on the grounds. This has often been done, and has been taken up by scientific men, who ought to know that it is a forlorn hope. Laying down breeding stock has much to be said in its favor in certain cases where the edible oyster is concerned, especially if the beds are in land-locked waters, where there is little chance of the spat being swept away by the tide; but it is too much to expect that any appreciable percentage of the larvæ of the mother-of-pearl oyster, which lives a pelagic life lasting for some days, if not weeks, and which occurs in places where the tidal currents are frequently five knots or more, would return to settle alongside their parents. Of course, the establishment of State breeding reserves, which Mr. Kent advocated in a report to the Queensland Government in 1905 is quite a different matter, and merits attention.

Breeding tanks were also installed, presumably with a view to rearing larvæ through the pelagic stage. I have already said that this is a project that has never been successfully realized, and proposals of this kind should now cease to be taken seriously, until at least experimental results have been demonstrated.

It appears that the production of so-called "pearls" was the chief object of this company. The process for producing these was kept a secret, and I believe the documents relating thereto are still in the possession of the successors of the company. There is, however, good reason for believing that the process was analogous to the Japanese and Linnean processes.

The "pearls" figured in the appendix to the report of the Queensland Pearl Shelling Commission (1908), and those figured by Saville-Kent in "The Great Barrier Reef," and "The Naturalist in Australia" certainly were. Mr. Saville-Kent once showed me some of these so-called "pearls." I have already given my opinion on the value of such blisters in the beginning of this paper. Their production on a commercial scale in Queensland would, I think, at the best be only practicable as an unimportant adjunct to the cultivation industry.

The weak points in this enterprise seem to have been the supposition that the "bastard" shell was the young of the mother-of-pearl oyster; the too-sanguine assumption that the latter could be bred in tanks; and the confusion between "blisters" and "pearls." The lease was much too short, and the law did not provide satisfactory redress against trespassers. The lease was abandoned in 1909, owing to Mr. Kent's death. Mr. Kent had also some concessions in the waters between Borneo and the Malay Peninsula; but I am not aware that they were developed.

(6) MR. T. H. HAYNES' EXPERIMENTS IN NORTH WEST AUSTRALIA.

In 1902, Mr. T. H. Haynes, an experienced pearl-sheller in Western Australia and the East Indies, obtained a concession covering the Montebello Islands, a well-known locality for *M. maxima*. At first he carried on the work as a private concern, but in 1909 the latter was taken over by a syndicate, the Montebello Shell Syndicate, Ltd. The first season in which work was carried on was 1904. A tidal pond, an acre in extent, was made by closing a natural bay with a wall and sluice. (See Figs. 7 and 8.) The pond could be emptied by specially constructed syphons. Between two hundred and three hundred shell were laid down in this pond, as breeding stock, and they throve well. No young shell appeared in the pond.

Further experiments were made in 1909 and 1910; but with inconclusive results.

A third and more elaborate experiment was made in the season 1910 and 1911. The particulars of this experiment, given below, were supplied to me by Mr. Haynes. Mr. Haynes had determined that in these waters the spatting season is between October and April. A breeding stock was provided consisting of oysters which had been acclimatized to the waters before the previous season. They were ascertained to have maturing gonads in November, 1910.

As a preliminary to the experiment the pond was emptied, and all fish ejected. Between three hundred and four hundred breeding oysters were introduced. The pond was closed, and the only change of water occurred by the falling of the level some nine inches on the ebb owing to percolation through the bottom, and by the restoration of an equal amount through the sluice on the flood-tide. Catchment of various kinds was provided. Examples from the breeding stock were examined from time to time, and the maturation of the gonads tested.

Young oysters, of a kind, appeared. The first were seen nineteen days after the breeding stock were laid down; a few were as much as one eighth of an inch in diameter. These and subsequent deposits of young oysters all died off. It is not possible to say whether they were produced by the oysters in the pond, or whether they were "bastard" shell, introduced in the water at flood tide. Mr. Haynes thinks they were young *M. maxima*, produced by his breeding stock, and it appears from a report by him (Mother-of-pearl Shell Culture, Report to the Directors of the Montebello Shell Syndicate, Ltd., London, 1912) that Mr. Dannevig, the Commonwealth Fisheries Officer, was inclined to share his view; but I am unable to agree with him, though not denying the possibility of his contention. Of course, without specimens it is useless to discuss what species they were; indeed, I know of no character which will allow of the identity of a species of *Margaritifera* being safely diagnosed, at so small a size. There is not, however, satisfactory proof that the breeding stock emitted spawn in the pond, and there is reason to think that at the close of the experiment they had not yet done so. And, although enough is not known of the duration of the free-swimming stage of this species to allow me to say whether the spat found nineteen days after the beginning of the experiment could have been produced in the tank, I lean to the view that this is unlikely, and that these young oysters, whatever they were, had been introduced with the water.

The experiments have for the present been abandoned. Up to date they have cost $34,000, not taking into account the personal services of Mr. Haynes.

The weak point in this work was that it was carried on without scientific assistance. While in a great many cases a scientific man, expected to undertake constructive work and to initiate operations on business lines, fails through lack of previous experience and business instinct, there can be no question that in a case like this, where initiative and resourcefulness, faith in the practicability of the enterprise, and practical and business knowledge were possessed in a marked degree by Mr. Haynes, the advice of a naturalist, concentrating his work on the practical problems which presented themselves, and refraining from dissipating his energy over the intensely fascinating field presented by an unworked tropical fauna, might have made all the difference. Indeed, I have often said to my friend Mr. Haynes that, had he and I had the luck to meet some eleven or twelve years ago, when I was in a position to undertake work of this kind, we should probably both have made our fortunes by now; or, if, as is so often the case, the originators of the enterprise had been "frozen out" and left stranded by the financial gentlemen who generally step in at a later stage, we should have, at any rate, the satisfaction of feeling that our names would go down to posterity as the founders of a new industry.

Other enterprises have been started in Australia, at Beagle Bay and elsewhere; but they have been largely empirical and are thus outside the scope of this paper.

(7) TRANSPLANTATION OF THE AUSTRALIAN MOTHER-OF-PEARL OYSTERS (*M. maxima*) TO THE PACIFIC.

A few years ago a Frenchman took about one hundred live mother-of-pearl oysters from Torres Straits to Noumea; but I have no knowledge what became of these.

In the year 1904 Levers Pacific Plantations, Ltd. (to which company I am indebted for much of this information) engaged Mr. Saville-Kent and transplanted fifteen hundred examples of *M. maxima* from Torres Straits to Suwarrow Island, a distance of about three thousand miles. The transport was carried out successfully, only a small percentage being lost. The oysters were laid down in the lagoon at Suwarrow, which already contained the black-lipped species. The secretary to the company informs me that the oysters did not become acclimatized or increase, but gradually died out. Large quantities of small shell were reported as growing on the marine grasses at Suwarrow, but these proved to be a worthless kind, and not the young of the introduced oysters.

The failure of this experiment was only to be expected, and serves to confirm the conclusions I arrived at after my experiments in 1899 and 1900 at the Conflict Atoll, that this species cannot profitably be introduced into atoll lagoons far from land or river influences.

Besides these actual experiments in the acclimatization of this species outside its natural haunts, various proposals have been made, casually or seriously. It is obvious that, if such a valuable animal as *M. maxima* could be introduced into a locality where it would become acclimatized and reproduce, it might become a very important new asset. There is no reason why there should not be localities where this species is not native, that possess the necessary conditions to enable it to be established. But, in view of the very special characters of its natural haunts, it would be necessary to treat such proposals with great caution. It appears from Mr. Haynes' report, referred to above, that at one time Mr. Crossland contemplated the introduction of twenty thousand West Australian mother-of-pearl oysters into the Red Sea. I think, however, that it is very doubtful whether this species would live in the Red Sea, where the density and salinity of the water are much higher than on its native grounds.

I understand that the introduction of this species into the West Indies has also been suggested; but it is to be hoped that before expenditure is incurred steps will be taken to obtain advice from some one competent to speak on the matter. The question of its introduction into Ceylon has also been discussed; but nothing has come of it.

When one considers the enormous potential asset that the mother-of-pearl fisheries are to Australia, scattered as they are all along her most vulnerable side, the north and northwest coasts, one is impelled to ask why more has not been done to develop them on lines which would result in the establishment of a permanent white man's industry. In the early days some of these grounds were enormously rich, carrying shell to the value of thousands of pounds to the square mile. These grounds have now been denuded, and fleets and vested interests, valued at hundreds of thousands of pounds, have been built up out of the proceeds of the exploitation of this natural wealth. The industry is now languishing, and is merely an asset for the Japanese and other aliens, save for a margin of profit made by the Europeans, who still finance and nominally control it.

One cannot but ask why Australian statesmen, so far-seeing where other kindred matters of policy are concerned, have not yet seriously invoked the aid of science. I think the reasons are probably twofold. Firstly, there is the effect such a change would have on existing vested interests. There can be no denying that any attempt to initiate conservation and cultivation would be most unwelcome to the present fleet owners, as it would certainly entail the closing of considerable areas of the grounds, and the substitution of individual for common rights thereon. Moreover, the success on any considerable scale of cultivation would be a severe blow to those whose money is invested in the industry as at present carried on, and who would be faced with the necessity of writing off large losses. Secondly, Australians, like the rest of the British people, have perhaps hardly yet realized the strength of zoölogy; that is to say, the immense amount of practical and theoretical information that is available, if it can only be properly mustered and co-ordinated for the elucidation of their problems. Against this potential strength of our science must be set off the drawback that work of this kind has hitherto been regarded as a fit training for the young and inexperienced man of promise, a useful stepping stone to a post at home, rather than as demanding the best men the empire can offer.

And yet, to anyone with imagination and faith in the possibilities of his subject, the Australian pearl fisheries offer work of a kind that seldom fails to the lot of a zoölogist. The man who can show how the old and formerly rich beds can be restored as an asset for the white man will be able to see, perhaps, not only as a vision but as a reality, the north and northwest coasts of Australia, which are crying out for settlers, peopled with men of his own race, somewhat scattered, perhaps, but none the worse for that, drawing a part of their living from the sea, a part from the soil. To the biologist who can solve the Australian problem there is in store not only the privilege of advancing knowledge and industry but the honor of being numbered among empire-builders.

A consideration of the small amount that we biologists have so far been able to do to improve the prospects of the pearl and mother-of-pearl fishing industries, and of the immense possibilities of these industries as a field for applied biology, leads one to inquire whether it would not be possible to devise a means for render-

ing our science more useful, and more directly available to those who may be disposed to invoke our aid. The following suggestion is therefore put forward, tentatively, for the consideration of those concerned with the organization of science.

Is it not time, in view of the minute and ever-increasing specialization of our subject, that some kind of machinery were provided that would, when required, bring together and make available for the public, whether governments, financiers, or shareholders, the available scientific knowledge and advice, on particular subjects such as problems of economic biology? It is seldom, when a new subject like this is broached, that the information necessary to achieve practical results is all in the possession of any one man. Such machinery, if it existed, would be a most valuable asset, never more needed than now, when investors are looking further and further afield for openings for their capital.

What seems to be needed is some organizing or co-ordinating machinery that will bring to bear on a question like this all available reputable specialist opinion that is likely to be useful, both in the preliminary stages, when a plan of campaign is being laid, and in the later phases, when examination, criticism, and correlation of results, and the formulation of a working policy, are required.

It is suggested that if such machinery existed, not only would the prospects of such missions as the Ceylon one, undertaken under the wing of a strong government, and backed at a later stage by abundant capital, be brighter, and some of the mistakes that have undoubtedly been made in the past be almost impossible, but the public would soon begin to realize that there were available expert "Courts of Appeal" to protect administrations and investors.

Without measures for correlating and concentrating specialist knowledge, the progress of economic biology, as it becomes more and more specialized, will run a risk of being seriously impeded by difficulties similar to those which baffled the builders of the Tower of Babel.

Note on the History and Significance of Biochemistry*

By Carl L. Arlsberg

The subject of biochemistry, has, I think, some general interest because originally no very definite distinction was made between biochemistry and any other kind of chemistry. One of the first real biochemists was Lavoisier, whom all matter, whether living or dead, interested. He performed the first calorimetric experiments. He was the inventor of the ice calorimeter, and showed that animal heat was the result of oxidation. All the chemists of that generation and the immediately succeeding one did biochemical work. I need only cite Liebig, who is perhaps in some ways the greatest of all biochemists. Unfortunately, about the latter part of Liebig's life chemists lost interest in biochemistry. This was due very largely to the sudden and tremendous development of organic chemistry, which was brought about by the discoveries of men like Hofmann and Kekulé. It was so easy to make new synthetic substances and thereby gain a sort of immortality, even though the main result of putting a chlorine atom here and a bromine atom there was to fill up Beilstein. In consequence, thoroughly trained chemists did not busy themselves with subjects that were really important in the elucidation of that matter which is found in living organisms, and which forms the physiological basis of life. The scientists in biology and medicine needed such information. The chemists did not give it to them. Consequently, physicians and physiologists who were ill-equipped for chemical research were forced to carry forward the work of biochemistry. Though the net result of their work made decidedly for progress, only too often it created confusion and artificial difficulties. Even the best biochemists of those days make us wonder why they did not pursue their chemical investigations as far as the chemical methods of that day would permit. The answer is, I think in many cases, that they were not real chemists but physiologists with a chemical veneer. Fortunately, this has been changing during the past decade, largely owing to the work of Emil Fischer. While we recognize in him a master of chemical technique, we may be certain that in a measure, at any rate, the preeminent position which he occupies among the chemists of his time is due to his clear conception of the really most important work in organic chemistry along biochemical lines. Fortunately, more and more organic chemists are following in his footsteps, and are devoting their attention to substances which occur in living things. I wish here to make a plea for more of this sort of work in America. I believe that the rewards and recognition for knowledge of chemistry applied in biochemistry are great, because the work of the biochemist will be applauded not merely by chemists, but also by zoologists, botanists and physicians. A biochemist has a wider audience because his work presents a more general appeal than the work of organic chemists upon such subjects as dyestuffs and the like. Further, I wish to point out the value of instruction in allied subjects. Not every organic chemist can successfully attack all biochemical problems. Because his organic chemistry, other experience in physiology, and above all, experience in dealing with substances which do not crystallize, are necessary. In many cases it is difficult to conduct biochemical research because the biochemist must very frequently begin with the smears, which the organic chemist consigns preferably to the slop jar. While the things which will not crystallize interest less the organic chemist, they are the very classes of substances with which the biochemist must deal. Great care, great patience and a knowledge of colloids are required of the organic chemist who wishes to work in biochemistry, but I feel confident that the reward for such men is great, not merely in pure science, but also in industries and in the arts.

The history of biochemistry in America is similar to that abroad. In America it developed first in the seventies and eighties in the medical schools of the country; and, at that time, it was controlled by physicians and physiologists abroad. The subject was narrowed to the consideration of biochemistry as affecting the life of man. That is to say, the chemical side of physiological processes of the human body together with such considerations of bacteriological chemistry as affect man in health and in disease. This phase of biochemistry is cared for very adequately and acceptably by the American Society of Biological Chemists, the first biochemical society to be formed in America.

The phase of biochemistry which the American Chemical Society can very naturally expect to encourage are quite distinct from the aims of the American Society of Biological Chemists. Our usefulness will include the biochemistry affecting agriculture, phytochemistry in particular, and such industrial processes as are based upon biochemical reactions. For example, the more exact study of the chemical composition of fruits, grains and food products. It must be admitted that, at present, we know only those chemical substances occurring in considerable amounts in such important grains as wheat and corn. The minor constituents in grains of much importance have not been identified with exactness. If we consider grains of less importance even this degree of knowledge cannot be claimed.

Some of our most important modern industries, like those dealing with starch, artificial fabrics, leather tanning materials, glue and gelatin, meat packing and the flour-milling industry require biochemists, and we are now training men to deal with such practical problems.

If our society confines itself to the activities already mentioned, there still remains a wide field of biochemistry uncared for, the biochemistry of the lower animals. This part of the biochemical work will become a part of the work in the zoological societies of the country. My view is that three societies of biological chemistry can well exist in America without competing in any way and each one caring for a specific need. These would include the biochemistry of the higher animals and its application to medicine; the biochemistry of the lower animals, and biochemistry in its application to plants, agriculture and the industries.

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The Longevity of Microbes

The numerous experiments of M. Trillat and of his pupils had already shown the influence of the presence of gaseous emanations in the air on the longevity of microbes. It is thus that M. Trillat has recognized that the impurities that are daily diffused in the atmosphere, and which proceed from the decomposition of organic matters of animal or vegetable origin, form an extremely favorable environment for the longevity of certain infinitely small microbes. Newly exhaled breath, the emanations of the earth, the neighborhood of bodies in a state of decomposition, are all sources of gaseous aliments for microbes. The experiments made by M. Trillat on the air of glaciers and air taken in the neighborhood of habitations have clearly demonstrated this influence. M. Trillat, in collaboration with M. Fouassier, has continued this study which has been presented to the Academy of Sciences by M. Laveran. The two scholars have just shown that the transport of pathogenous microbes in the air is effected especially by the damp which contains, in an infinitesimal state, traces of gas-aliments. Moreover, it would seem that the air when it fulfils certain conditions of dampness, of chemical composition, of temperature, and of age of microbes, is capable of being fertilized directly by the contact of a microbian source. Up till now it was thought, according to numerous observations of the German scholar Flügge, that for microbes to be transported by the air it was necessary to project them into it by some mechanical action, such as pulverization or any other means, the effect of which would be to detach them from their support. Contrary to this notion, MM. Trillat and Fouassier have established experiments demonstrating that when the super-position of certain factors takes place, the sowing of the air is performed in the same manner as that of a bouillon of culture, merely by the play and movement of the invisible vesicles which constitute the humidity of the air. In an infinitely feeble volume of about one hundred thousandth of a cubic millimeter these tiny drops are uninfluenced by the action of the force of gravity. They are always mobile under the influence of the least variation of temperature. All these results are of the greatest interest for bacteriological science. MM. Trillat and Fouassier have shown how the contamination of the air takes place in a closed and tranquil space, without the intervention of the presence of dust or of any mechanical means, as was believed up to the present time.—*Chemical News*.

SCIENTIFIC AMERICAN SUPPLEMENT

Founded 1876

NEW YORK, SATURDAY, JANUARY 3, 1914

Published weekly by
Munn & Company, Incorporated, Charles Allen Munn, President
Frederick Converse Beach, Secretary and Treasurer
all at 361 Broadway, New York

Entered at Post Office of New York, N. Y., as Second Class Matter

The Scientific American Publications

Scientific American Supplement (established 1876)	per year	$5.00
Scientific American (established 1845)	"	3.00
American Homes and Gardens	"	3.00

The combined subscription rates and rates to foreign countries including Canada, will be furnished upon application

Remit by postal or express money order, bank draft or check

Munn & Co., Inc., 361 Broadway, New York

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Table of Contents

	Page
Heredity.—By W. Batson	2
Darwinism One Hundred Years Ago	3
Construction of a 50-watt Transformer.—By Charles F. Frasser, Jr.—13 illustrations	4
Over-crowded Street Cars	5
Use of Ice and Other Means of Preserving Food in Homes.—By John R. Williams, M.D.—1 illustration	6
Economy of Ocean Transport	7
Rate at Which Time of Trains Can Be Made Up.—By Paul M. La Bach.—1 illustration	7
Electric Light and Eyesight	7
Hardening and Tempering Steel.—10 illustrations	8
Development of Water Power in Nova Scotia	9
Natural Gaseous Mixtures Rich in Helium	9
On the Origin of Solar Electricity.—By J. A. Harker	10
Naval Expenditure of Powers.—1 illustration	11
The Modern Blast Furnace Burden	11
The Pearling Industry.—By H. Lyster Jameson.—8 illustrations	12
Note on the History and Significance of Biochemistry.—By Carl L. Arlsberg	16
The Longevity of Microbes	16